The Collected Works of Peter Razzell

An Anthology

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Table of Contents

Introduction 0

Social Origins of Officers in the Indian and British Home Army, 1758-1962 4

Edward Jenner: The History of a Medical Myth 17

Population Change in Eighteenth-Century England 31

Population Growth and Economic Change in 18th and Early 19th Century England and Ireland 53

Statistics and English Historical Sociology 75

Introduction to the Victorian Working Class 95

The Pools Winners 107

Edward Jenner's Cowpox Vaccine: The History of a Medical Myth 197

The Protestant Ethic and the Spirit of Capitalism 333

Introduction

I have included in this collection all my writings and publications since my first publication in 1963. Recent publications include two new papers on Shakespeare. They cover radical new ideas about his biography, including evidence on how he achieved the cultural knowledge to write the plays, as well as explaining his whereabouts during the so-called "lost years".

My new paper – 'Population growth and the development of capitalism in England, 1550-1850' – summarizes my work and thinking on the role of population change in economic development, presenting a thesis of 'demographic determinism', shaped by disease patterns during a period of three centuries.[1]

I graduated with a degree in social science from Birmingham University in 1961, specializing in sociology. My first publication – *The Social Origins of Army Officers* – was based on research carried out at Chicago University and laid the foundation for much of my subsequent work. I had found that there was a significant increase in the number of gentry and aristocratic officers entering the British army during the late eighteenth and early nineteenth century. This appeared to be linked to a major growth in life expectancy among these groups from the mid-eighteenth century onwards.

This raised the question of why mortality had reduced among the wealthy and led me to explore the role of smallpox inoculation, which formed the basis of my Ph.D. thesis. However, subsequent research indicated that inoculation could only be one of the reasons for the reduction in the death rate, as new evidence revealed that there was major fall in adult mortality in England – approximately halving between the beginning and end of the eighteenth century – well before the widespread practice of inoculation. Given that the fall in the number of deaths occurred among all socio-economic groups in all areas of the country, this suggested that there had been an exogenous reduction in overall disease virulence.

Nevertheless, there is evidence that case fatality rates of one disease – smallpox – increased very significantly during the period between the sixteenth and the nineteenth century. For example, less than five per cent of children died from the disease in London during the sixteenth century, whereas by the end of the nineteenth this had increased to nearly forty-five per cent among children unprotected by vaccination. However, the pattern of smallpox mortality is very complex: it was a disease of both adults and children in the south of England but affected only young children in the north. Case fatality rates were much higher among very young children and elderly adults, making it difficult to assess overall mortality. Inoculation was practised much more widely in the south of England than in the north, probably as a result of panic responses from adults who were affected by the disease.

This website also includes specific studies of smallpox, such as its impact on average height, as well as the reduction of adult smallpox mortality in London during the late eighteenth century. Additionally, I have published evidence to show that Jenner's

[1] For an up-to-date and comprehensive analysis of the role of disease in history see J. Kennedy, *Pathogenesis: How Germs Made History*, 2023.

vaccination was a more attenuated form of the old inoculation, a topic requiring very detailed and complex research.

My subsequent work indicated that overall infant and child mortality fell from the mid-eighteenth century onwards, first among the wealthy and only later among the general population. This was probably the result of a number of factors: the practice of smallpox inoculation, better personal hygiene and improved environmental and midwifery practices. However, by the late nineteenth century there appears to have been little or no social gradient in infant mortality, although it did exist among young children. Additionally, there is limited evidence to suggest that there was a similar lack of a class gradient amongst adults in the 1880s, although such gradients did emerge in the twentieth century both for infants and adults.

My research on the Hertfordshire Health Visitors Register used by Barker in his work on his 'fetal origins' hypothesis indicates that there was no correlation between poverty and birth weight in the 1920s and 1930s, although there was a strong association between poverty and weight gain in the first year of life.

Although Malthus in his theoretical writings emphasized the impact of economic changes on fertility levels, in his work on England he concluded that the prime reason for England's population growth in the eighteenth and early nineteenth century was a reduction in mortality, due to a decline in infectious diseases and improved personal and public hygiene.

My research has largely confirmed the above conclusions, although the pattern of mortality and fertility change appears to be more complex than was previously envisaged. The age of marriage of women in England in the first half of the eighteenth century was significantly higher amongst poor labourers than amongst the wealthy, but this changed radically after the middle of the century. The aristocracy, gentry and other wealthy groups began to marry much later, and the poor much earlier, so that by the end of the nineteenth century there was a marked socio-economic gradient in marriage ages.

Research using a number of different sources indicates that marriage in England was nearly universal during the sixteenth and seventeenth centuries. However, the proportion of women marrying declined significantly in the eighteenth century onwards. This was linked to socio-economic status and the growth of literacy, which had an impact on fertility levels. By the early nineteenth century fertility was much higher amongst the poor than the wealthy, indicating that the fertility transition first took place amongst the wealthy in the eighteenth century.

These marriage patterns were probably largely the result of the mortality changes summarized above. The wealthy no longer had to marry very early and so frequently because of the significant reduction in mortality, whereas the number of labourers had increased as a result of growing life expectancy, resulting in a surplus of labour and increased poverty and pauperization. Malthus described the need for an improved living standard for labourers in order to achieve the higher aspirations and literacy necessary for the reduction of early marriage.

I have explored in detail the population history of England, focussing on the reliability of parish register data. Historical demography like other social sciences – including economic history – became increasingly focussed from the 1960s onwards on the use of mathematical models. My research led me to conclude that these models were flawed because of the unrealistic assumptions that they made about key variables, and that the only way to address this problem was to adopt a strategy of methodological triangulation. This was the main method used for evaluating parish register reliability. It was also used to address the problem of measuring socio-economic status: there are several papers on the website that deal with this issue by comparing occupational classifications of individual families with the number of domestic servants, the rateable value of households and the use of private/public doctors.

I accepted that social science was a part of the natural sciences – see my papers on *The Problem of Determinism*, Max Weber's *Protestant Ethic*, as well as *Max Weber and Environmental Determinism* on this website – but there needed to be a rigorous examination of the reliability of all variables when attempting to explain historical phenomena.

My research indicates that mortality rather than fertility was the key variable for explaining population change, and that this was fuelled by shifts in disease patterns rather than variations in per capita consumption of food. This led to the conclusion that population was largely an exogenous variable in economic and social development, a theme explored in a number of the publications and writings on this website.

This conclusion has radical implications for the study of both economic and social history. A well-documented example is the impact of plague on medieval society. The shortage of labour in the fourteenth century forced up wages, and the attempt of the government to restrain wages was probably a major factor in the peasant's revolt during that period.

The role of surplus labour in the growth of capitalism has long been recognized, but this was seen by Marx and others as a result of economic development. By contrast, the demographic creation of labour surpluses through population growth played a key role in capitalist development in England during the seventeenth and the nineteenth centuries. These were periods when an excess of labour drove down wages and increased prices and the wealth of the owners of capital, creating both an increase in socio-economic inequality and political change.

A number of papers on this website addresses this theme. During the late sixteenth and first half of the seventeenth century population had grown largely as a result of the gradual disappearance of plague. This led to increasing property prices due to increased demand for food and other consumer goods. There was a marked rise in the wealth of yeomen farmers at this time, and along with tradesmen they became increasingly literate. These groups formed the backbone of Cromwell's New Model Army, playing a major role in the English civil war.

Traditionally England had not relied on a standing army but used the navy as the chief form of defence against external attacks. This was possible because of its

geographical position as an island, which had a major impact on its economic and social history. On the continent of Europe, standing armies had developed because of the threat of land-based attacks, which strengthened authoritarian regimes and the power of monarchies. English kings relied on militias, which resulted in a limited ability to impose taxes and control the economy. As a result, a culture of individualism grew up, particularly in areas outside the manorial control of the aristocracy and gentry.

My book on Shakespeare can also be seen in the context of these developments. Shakespeare participated in some of the economic activities of his father, who exploited current economic conditions in the late sixteenth century by illegally practising money lending, the sale of wool, and the hoarding of grain. Such traders were a part of a sophisticated group of independent merchants who travelled widely throughout England, putting on plays in inns and other venues and providing the cultural background for the writing of Shakespeare's plays.

The later period of the late eighteenth and early nineteenth century saw changes in access to elite occupations and the increasing pauperization of labourers as a result of population growth. This resulted in the growth of inequality and political radicalism during the French revolutionary period.

I have also presented evidence to show that current world population developments in Asia have had a major impact on economic and political conditions in England, Europe and the United States. Mortality fell sharply in China and elsewhere in Asia in spite of famines from the 1970s onwards, mainly because of the application of modern medicine and the growth of improved personal and public hygiene. This resulted in an explosion of population, which provided the basis for surplus labour, exploited by Asian companies to manufacture cheap goods and export them to European and American countries. That has led to the erosion of manufacturing industries in these countries, resulting in economic inequality and the rise of populism in rustbelt areas.

Demography has been seen traditionally by economists and other social scientists as a function of economics, but this website presents detailed evidence to show that it has acted as an independent force largely through changes in disease patterns, helping to shape the world's economic and social development.

Peter Razzell

SOCIAL ORIGINS OF OFFICERS IN THE INDIAN AND BRITISH HOME ARMY: 1758–1962

P. E. Razzell

THIS PAPER[1] may be viewed in two ways: firstly as an attempt to test certain sociological hypotheses and secondly as a contribution to the social history of Great Britain.

The sociological hypotheses are derived from work done by Professor M. Janowitz. They may be summarized by the following quotations:

> From an historical and theoretical point of view, there was every reason to believe that the military would be heavily recruited from non-industrialized areas—from agricultural areas and small towns . . . most fundamental there has been an integral association between military institutions and rural society. But in the final analysis the link between rural social structures and military organization is based on the more central issue of career opportunities.[2]

> The broadening of the social base of recruitment (in the nineteenth century) took place in all the European countries, although the rate varied to some extent from nation to nation.[3]

> The aristocracy first gave way to the middle classes in the artillery and the technical services where specialized technical training was required. In the more honorific cavalry, with its natural link to feudal life, the upper social stratum concentrated its numbers in the face of military expansion.[4]

> In England the professional military and its élite members were mainly recruited from the southern rural counties (for the period 1914–50).[5]

In order to test these hypotheses, I initially studied British officers in the Indian army.[6]

As can be seen from Table 1, during the late eighteenth and early nineteenth centuries the large majority of Indian army officers came from the middle class. However, the remarkable thing about this table is the *increasing* proportion of aristocracy and landed gentry over time —the percentage trebled during our eighty year period. A further breakdown of the statistics suggest that the proportion of the landed upper classes had stabilized itself by the end of the period.

TABLE 1

Changing Status Group* of Indian Army Officers

Period Arrived	Aristocracy	Landed Gentry	Middle Class	Number
	%	%	%	
1758–1774	1½	6	92½	448
1775–1804	3	13½	83½	626
1805–1834	5	19	76	950
Total	4	14	82	2,024

* Status Groups are derived from Burke's *Peerage, Landed Gentry*, etc.; Middle Class is a residual category.

TABLE 2

Percentage of Officers in the Indian Army with a certain type of birthplace: 1758–1834

	Actual percentage	Number	Expected* percentage
Rural	55	(588)	80
Small town	8	(89)	3½
Medium town	5	(56)	3½
Large town	34	(376)	14
	100	(1,109)	100

Rural = 3,000 people or below. Small town = 3,000–10,000 people. Medium town = 10,000–25,000 people. Large town = above 25,000 people.
*Taken from the 1801 Census.

Table 2 clearly demonstrates the over-representation of urban areas, especially the large towns. If plotted over time, place of birth shows little apparent trend, but against the background of the industrial revolution and concomitant urbanization during the period, the trend appears as an increasing proportion of men from rural areas and fits in with the increase in recruitment from the landed upper classes. It should be noted that there is a slight overall majority of officers from rural areas.

Table 3 shows that a disproportionate number of officers came from London, Wales and Scotland. We also ought to include Southern Ireland, because much of its population was not allowed into British armies as officers because of their Roman Catholic religion. Further analysis shows that Scotland, Ireland, Wales and the N.W. of England supplied more than their share of landed upper classes—certainly all the

Border Countries must have had large numbers of redundant 'gentlemen'. It is significant that the large towns and cities appearing in the study are invariably old and traditional ones, for example, Edinburgh rather than Glasgow, London rather than Birmingham or Manchester.

TABLE 3

Officers in the Indian Army by Region : 1758–1834

Country or Region	Actual percentage	No.	Expected* percentage
London	11½	(196)	6
Midlands	12	(170)	21½
S.E. England	13½	(188)	10
S.W. England	5	(70)	6½
N.W. England	4½	(58)	13½
S. Ireland	15	(214)	18½
Ulster	4½	(64)	9
Wales	6	(75)	3
Scotland	25	(353)	11
Total	100	(1,388)	100

*Taken from the 1801 Census.

It is the industrializing rather than the urban areas which are under-represented in the Indian army. At this point it is interesting to ask what factors are important in making certain regions and countries good recruiting areas. Is it religion, ecology or occupational alternatives? Professor Janowitz in his book indicated that there was a link between the conservative hierarchical religions and armies in the United States. My data indicate no such relationship for the Indian army. Scotland is largely over-represented, yet its established religion is Presbyterian, the most individualistic non-conformist religion of all.

TABLE 4

Status Group and Branch of Service of the Indian Army : 1758–1834

	Cavalry	Infantry	Artillery	Engineers	No.
	%	%	%	%	
Aristocracy	13	65	15	7	75
Landed Gentry	8	75	12	5	285
Middle Class	5	83	10	2	1,664
Total Group	6	81	10½	3	2,024

Also if we allow for the 50 per cent of Ulster's population who are non-recruitable Catholics, this province has the proportion expected of it, even though over half of its Protestant population is Presbyterian, which is not the established religion of Ulster.

As for ecology, Table No. 4 and related statistics are relevant.

From Table 4 it may be seen that, although there are more of the landed upper classes in the cavalry than one would expect, the same thing is even truer for the engineers and artillery (the infantry is the 'social outcast' of the service to some extent); there is little change in this pattern over time. Further analysis of data not produced here shows that, surprisingly, there are twice as many cavalry officers from London as from the average region, and virtually no engineering officers from the Midlands or the N.W. of England. Also there are more sons of merchants in the cavalry, and more sons of industrialists in the engineers than would be expected statistically. Similar to this finding is that urban areas are over-represented in the cavalry, while such areas are under-represented in the engineers. Thus we see none of 'the integral association between military institutions and rural life' or any 'natural link (of the cavalry) to feudal life'.

As for occupational alternatives, I looked for some sort of relationship between the agriculture of a region and the numbers of landed officers from it. In fact the regions were defined partly on the basis of

TABLE 5

Status Group and Rank in the Indian Army : 1758–1834

	Aristocracy	Landed Gentry	Middle Class	No.
	%	%	%	
Lieutenant and below	3½	13	82½	922
Captain or Major	3½	10½	85	660
Lt.-Col. or Colonel	3	22	74	305
Maj.-General and above	6	13	79	137
Total all Ranks	4	14	82	2,024

enclosure of land, i.e. the Midlands were a central belt running north–south of counties which contained a high proportion of land unenclosed at the beginning of the eighteenth century.[7] The Midlands were under-represented in supplying the upper landed classes as officer recruits, whereas N.W. England[8] is considerably over-represented. Though data are far too vague to draw any definite conclusions, it may be suggested that, as both areas contained industrial pockets, differences in agriculture are possibly one of the main causes of differences in

the supply of landed recruits, i.e. the enclosing of land might have supplied new estates for increasing numbers of younger sons. This finding, taken in conjunction with the previous one about the under-representation of industrializing areas, leads us to agree with Professor Janowitz about the central importance of occupational alternatives.

Table 5 shows a slight association between rank in the army and social status, with the aristocracy in the very top ranks and the landed gentry in the upper-middle rank of colonel. However, when length of service is taken into account, it appears that the aristocracy serve for longer periods, the landed gentry for more medium periods, and the middle class for shorter periods. Rank correlates with few other variables —an exception being that sons of the military tend to occupy higher ranks. Overall the picture is one of little social bias in promotion to the top positions.

TABLE 6

Average (median) length of service in the Indian Army

Period	Years	No.
1758–1774	8	448
1775–1804	19	626
1805–1834	17	950
Total		2,024

It is clear from Table 6 that the average length of service doubled within our period. This points to the growth of professionalism within the army. The wide social base of recruitment and lack of social bias in promotion also support this interpretation. So too does the fact that, during the first few years of the period 1758–1764, there were a considerable number of foreigners recruited into the army—in fact 10½ per cent of the total; this slumped to 1½ per cent by the next decade and never increased beyond this minor proportion—this suggests the decline of a mercenary element. Also the sharp increase in self-recruitment within the military tends to support this view.

To sum up the findings about the Indian army: they do not support the sociological hypotheses and if anything tend to refute them. Is this an exception to a general rule? An examination of the British home army might throw a little further light on the problem.

A comparison of Table 7 with Table 1 shows some striking differences. The British home army was very much more aristocratic than the Indian army—a ratio of something like 10 to 1 during the relevant period, 1758–1834. Also there are many more landed gentry in the home army—a ratio of about 2½ to 1. It should be noted that in the

TABLE 7

Status Group of Officers in British Home Army

	Aristocracy	Landed Gentry	Middle Class	No.
	%	%	%	
1780*	24	16	60	100
1830	21	32	47	100
1875	18	32	50	100
1912	9	32	59	100
1930	5	6	89	100
1952	3	2	95	100

* Each date represents an army list, from which samples were taken.

home army during the first period, the aristocracy actually exceed the proportion of landed gentry; this never happens in the Indian army where the gentry always substantially exceed the proportion of aristocracy. On the other hand, an interesting similarity between the two tables is the influx of landed gentry during the period 1780–1830. The increase in proportion of landed gentry in the home army is even more significant if we take into account the increase in the size of the officer corps, which more than doubled between 1780 and 1830—there was a five-fold increase in the numbers of landed gentry entering the army during this period. This is an important finding and will be discussed later. There are other significant things about Table 7. Between 1830 and 1912 the landed gentry achieved a remarkable stability in supplying recruits to the army; however there was an extremely sharp drop in the percentage of landed gentry officers after the First World War. The

TABLE 8

Status Group of Major-Generals and Ranks above, in the Home Army

Date	Rank	Aristocracy	Landed Gentry	Middle Class	No.*
		%	%	%	
1830	Generals	70	8	22	13
	Major-Generals and above	57	32	11	50
1912	Generals	30	22	48	19
	Major-Generals and above	24	40	36	50
1930	Generals	25	25	50	16
	Major-Generals and above	14	26	60	50
1952	Generals	22	17	61	18
	Major-Generals and above	3	2	95	50

* Total number of all Generals; weighted sample of Major Generals and above.

aristocracy, however, declined slowly throughout the period 1780–1952 although there is a fairly sharp drop in 1875, probably due to the abolition of the purchase system in 1871. The middle class occupy about one half of officer posts throughout the whole of the nineteenth century—a reflection of the relatively open class system.

The significance of Table 8 is seen when it is compared to Table 7. There are $2\frac{1}{2}$ times as many aristocrats in the ranks of Major-General and above as one would expect from the proportion of aristocrats in the whole corps for 1830, 1912 and 1930. This means that the aristocracy maintained their relative monopoly of top ranks, although they lost an absolute monopoly throughout the nineteenth and early twentieth centuries (note that the landed gentry were under-represented in the very top rank of General until 1930, when they were extremely over-represented: they were always well represented in the ranks of Major-General and above except in 1952: they certainly improved their position throughout the nineteenth and early twentieth centuries). The sharp slump in the proportion of landed gentry in the whole officer corps during 1914–30 is reflected in the slump of landed gentry in the ranks of Major-General and above during 1930–52. In fact the latter slump is even sharper than is shown in the figures, as none of the upper landed classes filled the rank of Major-General in 1952, but occupied the very highest positions (especially that of Field-Marshal). Most of the famous Generals in the Second World War were from the landed upper classes, for example, Montgomery (Irish landed gentry), Alexander (Irish aristocracy), Ironside (Scottish landed gentry). We see here a continuation of the tradition that the landed upper classes came from Border Countries and we cannot agree with Janowitz that 'the professional military and its élite members were mainly recruited from the southern rural counties'.

TABLE 9

Officers in the Home Army of Rank Major-General and above with inherited aristocratic titles attached to their names

Year	%	Number*
1780	30	155
1810	27	390
1830	27	509
1852	19	344
1875	9	500
1912	9	190
1930	2	146
1952	1	130

* Total number of Major-Generals and above in any one year.

Table 9 shows what happened to the titled aristocracy in top ranks as opposed to the aristocracy as a whole (the latter includes members of cadet, i.e. branch families). Surprisingly there was a considerable drop in the proportion of titled Major-Generals and above during the period 1830–52—surprising because of the usual historical interpretation, i.e. that the broadening of the social base of the army was due to the introduction of examination requirements in 1849 and the abolition of the purchase system in 1871. However the puzzle as to how the aristocracy maintained their relative advantage in top positions in spite of the abolition of purchase and adoption of the principle of seniority is solved by looking at what happened to the total number of Major-Generals and ranks above. There was a very sharp contraction in the total number between 1875 and 1912: the main effect of abolition was to clear much of the lumber from the top although middle-class lumber rather than aristocratic. The sharp drop from 1912 to 1930 of aristocrats with inherited titles reinforces the idea that the lower aristocracy and landed gentry took over the top ranks during this period.

TABLE 10

Officers with Inherited titles in various Regiments and Branches of the Home Army

	1780	1810	1830	1852	1875	1912	1930	1952	1962
	%	%	%	%	%	%	%	%	%
1st Life Guards	0	4	15	24	34	42	=15	11	3
2nd ,, ,,	17	15	21	24	27	21			
Royal Horse Guards	6	3	15	27	33	33	30	9	14
Grenadier Guards	15	17	13	20	23	23	—	—	—
Coldstream Guards	15	15	16	28	33	12	—	—	—
Scots Fusiliers	8	15	15	21	24	19	—	—	—
Artillery	½	½	2½	1½	1	½	—	—	—
Engineers	0	1½	1	1	1	0	—	—	—
Royal Marines	1	½	1	0	0	0	—	—	—
Total Army	2½	2	2½	2	2½	2	—	—	—

We see from Table 10 that all the Guards regiments became more 'exclusive' throughout the nineteenth century.

What was happening was that although the aristocracy was losing its monopoly of high rank, it maintained its social status by excluding outsiders from élite regiments i.e. the more power it lost, the more it attempted to maintain its status. This can be seen by looking at other branches of the army. Although the engineers, artillery and marines always had fewer aristocrats (an exception to this is the artillery in 1830), the proportion dropped throughout the nineteenth century.

At the beginning of the century the aristocrats were distributed more evenly throughout the whole army—usually in top positions in all regiments. By the end of the century they tended to crowd together in the regimental havens of social security. One of the most interesting features about Table 10 is the change that took place between 1875 and 1912: there was a sharp slump in the proportion of titled officers in the Coldstream Guards (from 33 per cent to 12 per cent), but a substantial increase in the 1st Life Guards—in fact, this regiment by 1912 was the most exclusive ever found at any time. It consisted of 70 per cent aristocracy, 18 per cent landed gentry and 12 per cent middle class. The First World War brought about a reorganization of the army and some regiments disappeared; of the rest only the Life Guards and Royal Horse Guards retained their exclusive character. Thus the First World War was also a watershed in regimental exclusiveness, although even today the Royal Horse Guards can muster 14 per cent titled aristocrats. It is easy to see the differences between the British home army and the Indian army with respect to regimental exclusiveness. There was virtually no exclusiveness in the Indian regiments and what little there was, was to be found in the engineers and artillery, as well as the cavalry. The home army was completely different: a small number of regiments were very exclusive, especially the cavalry—the engineers, artillery and marines were all shunned by the aristocracy.

Thus the home army accorded more with our sociological hypotheses, although there are exceptions here, for example, the relatively unchanging social composition of the home army during the nineteenth century. The value of such general hypotheses is questioned in the light of the complexities of our historical material.

I now turn to a discussion of the implications of the results for social history. First as implied, the sociological nature of the Indian army and home army officers was completely different. The reasons for this are probably not too difficult to find: the Indian army was the army of merchants (the East India Company) who would naturally tend towards bias against the aristocratic principle. Further, the aristocracy themselves were averse to service abroad especially in hot, humid climates. Cecil Woodham-Smith[9] describes the evasion of overseas service by the home army social élite in the following way:

> By going on half pay, or by exchanging at a price, into another regiment, wealthy officers avoided uncomfortable service abroad. When a fashionable regiment had to do a tour of duty in India, it was notorious that a different set of officers went out from those who had been on duty at St. James's Palace or the Brighton Pavilion. When the regiment returned, the Indian army officers dropped out and a smarter set took their place.

The aristocracy used the same techniques of going on half pay and

transferring into other regiments in order to win quick and easy promotion. They would buy the rank of captain (say) and a few months later transfer to half-pay (temporary retirement); then they would wait the required number of years before buying the post of major in a regiment where there was a vacancy (this of course was a check on regimental exclusiveness). It was in this manner that the Earl of Cardigan and the Earl of Lucan became generals, even though they had only spent eight years or so in full regimental service. The aristocracy at home did not earn their higher-ranking positions by longer service, as was the case in India. Woodham-Smith also describes the conflict which took place between the home army aristocratic élite and 'Indian' officers (officers who had seen service in India) during the Crimean War. The aristocracy viewed the 'Indian' officers with social contempt, whereas the latter were critical of the military capabilities of their aristocratic commanders. An example of the aristocratic approach is furnished by Lord Cardigan who in the Crimea refused to allow some of his horse lines to be moved to dry ground from a muddy patch where the animals stood knee-deep in mire, because it would spoil their symmetry.[10] The 'Indian' officers fared very much better—they were very effective as troop and company commanders, especially of Turkish irregular units. The reasons for their superiority are perhaps their extensive experience of war in India, as well as the promotion for ability rather than for social status. The Duke of Wellington, although a fervent believer in an aristocratic army, served in India for some years before his rise to fame.

In both the British home army and the Indian army there was an influx of landed gentry at the end of the eighteenth century and the beginning of the nineteenth: during the same period there was a significant growth of religious seriousness amongst the landed upper classes and the reform of the Public Schools. T. W. Bamford has demonstrated the concentration of landed gentry in Rugby. He also quotes Anne Merivale thus: 'Rugby was flourishing in numbers and reputation and aristocrats tried, and tried in vain, to make him open its doors for the admission of pupils from the higher classes.' Apparently Thomas Arnold was aware of the 'barbarian' nature of the aristocracy and wished to set and keep the serious religious tone of his school, so that he might educate Christian Gentlemen. Bamford also writes 'that there has been a persistent story that Rugby's success was built on the influx of the sons of manufacturers', although his data do not support this notion. All this seems somehow mysterious. Perhaps the role of the landed gentry in the revolution of manners, morals and society at the end of the eighteenth and the beginning of the nineteenth centuries, has been neglected. Analysis of the Indian army data showed that a considerable amount of 'social mixing' took place during the beginning of the nineteenth century: for example, we find a few landed

gentry whose fathers were merchants and even artisans during the late period, but not during the earlier times, though there were no aristocratic merchants; also over half of the landed gentry families (in the Indian army statistics) were new ones, whereas practically all the landed gentry in Scotland and the aristocracy everywhere were old families (surprising in view of the large number of Peerages created by George III). Possibly many old families faced the problem of how to place their increasing numbers of younger sons in suitable positions: evidence for this is to be found in the Indian army where during the earlier period a majority of officers were eldest sons, especially amongst the landed upper classes, whereas by the end of the period everyone is a younger son. There also appears to have been an influx of the middle class, especially merchants, into the landed gentry—perhaps those mysterious manufacturers' sons at Rugby? I believe that there was a fusion between the 'barbarian' gentry and 'philistine' middle class in the reformed public schools to produce the 'barbarian-philistine' Christian Gentleman, a hybrid whose personality has profoundly affected the English national character! Initially this transformation would be linked to the evangelical religious revival but later this would be diluted in the Public schools. The intellectual aristocracy described by Noel Annan[12] is derived from this upper-middle class. If this transformation occurred as described, the reasons for such changes were probably partly economic and demographic: a newly enriched merchant and capitalist class plus an over-populated gentry—over-populated enough to drive many a gentleman to the colonies and even India. The gentry were certainly transformed from local militia-men to officers in a standing army.

For the bourgeoisie, entry into the army must have been experienced as a considerable rise in social status: as an officer's pay at home had hardly changed from the end of the seventeenth century and the price of a commission was very expensive, (for example, the normal price of a cavalry lieutenant-colonelcy during the 1860's was £14,000),[13] only the wealthy could afford a career in the army, and the wealthy at that time were usually from the landed upper classes. The army had been a great conservative force in British life. Thus the Duke of Wellington could say, 'It is promotion by purchase which brings into the Service . . . men who have some connection with the interests and fortunes of the country . . . It is this circumstance which exempts the British army from the character of being a "mercenary army"; it has rendered its employment for nearly a century and a half not only not inconsistent with the constitutional privileges of the country, but safe and beneficial . . . Three-fourths of the officers receive but little for their service besides the honour of serving the King.'[14] This honour was sufficiently great to create a black market which dealt in commissions. Permission to purchase a commission was very hard to come by and there existed a

recognized auction room for dealings in commissions in Charles Street, London, where competition was often keen. Black market prices were roughly twice the officially stipulated prices. The Duke of York's mistress, Mrs. Clarke, created quite a scandal in 1807 when she was found to be involved in this black market. This system was successful unlike the French one described by Eleanor Barber[15] where the bourgeoisie were excluded from the army, as well as from the civil service and church—she maintains that this exclusion from social status was a contributary factor in the French Revolution.[16] In Britain the emphasis has always been on money, and the purchase system ensured that the British army was never closed to the wealthy middle classes —the Indian army allowed lesser lights to quench their thirst for social status.

Perhaps the most surprising finding of the study was the way the landed upper classes maintained their position within the army throughout the nineteenth century and even into the twentieth. The great watershed was of course the First World War. After this war members of the landed classes were remnants; although very important remnants. The exact reasons for the decline of the landed upper classes in the army at this time (1912–30) are rather difficult to find. Probably the elimination of estates through taxation and agricultural depression was one reason. Another might be the decline in status of the army as a career. Before 1914 a career in the army had been a leisurely avocation—plenty of sport, especially riding and hunting: truly an occupation for a gentleman. The nightmare of the First World War changed all this: the army had become a grim employer. Even so, the British army is far from being a democratically recruited one: figures published by the War Office[17] show that 80 per cent of Sandhurst Commissions during the 1950's went to public school boys, in spite of the fact that only about 10 per cent of the relevant age-group goes to these schools. Certainly the great public schools lost their monopoly of Sandhurst commissions: in 1891, 55 well-known public schools and universities supplied the total of 373 cadets, whereas by 1961 roughly twice as many cadets came from nearly six times as many schools (308), widely spread over the country and differing greatly in their form, size and status.[18] In fact the decline of the landed gentry coincided with the decline of the large, well-known public schools in the army. It is during this period (1891–1961) that education rather than land-ownership became decisive as the defining criteria of status groups. It is interesting to trace the decline of the landed upper classes—it is a structural process. The top status group, the aristocracy, goes first and is gradually replaced by the second, the landed gentry; it in turn is displaced by its own offspring, the public school graduate. Accompanying this downward decline is a drift to higher and higher positions by the remnants of the previously powerful status group, for example, the remnants of the

landed gentry were to be found in the rank of Field-Marshal by the 1950's. Perhaps we can expect the same thing to happen to the public school monopoly of top positions, but the sun has yet to set on the English Gentleman.

NOTES

[1] This paper originated as a research project under the auspices of Professor M. Janowitz, and under the financial patronage of the National Opinion Research Centre at Chicago. I would like to thank them for the help they gave me. Also Miss J. Klein and A. H. Halsey for reading the final draft of the paper.

[2] Professor Morris Janowitz, *The Professional Soldier: A Political and Social Portrait*, p. 85.

[3] Ibid., p. 93.

[4] Ibid., p. 95.

[5] Ibid., p. 95.

[6] The source of this data is Major V. C. P. Hodson's 'List of the officers of the Bengal Army, 1758–1832'. As Major Hodson spent over 30 years compiling this biographical and genealogical list, the data may be considered to be very reliable.

[7] Midlands as defined here included: Norfolk, Cambs., Hunts, Beds., Bucks, Oxon, Gloucs., Berks, Wilts, Warws., Northants, Leics., Rutland, Notts, Derbys, Lincs and Yorks.

[8] N.W. England is the other non-geographical category; it included Herefordshire, Salop, Staffs, Worcs., Ches., Lancs, Westmorland, Durham, Cumberland, and Northumberland.

[9] Cecil Woodham-Smith, *The Reason Why*.

[10] Colonel H. De Watteville, *The British Soldier: His Daily Life from Tudor to Modern Times*, op. 198.

[11] T. W. Bamford, *British Journal of Sociology*, Sept. 1961.

[12] Noel Annan: see the chapter 'The Intellectual Aristocracy' in *Studies in Social History*, ed. by J. H. Plumb.

[13] H. De Watteville, *The British Soldier . . .*, p. 177.

[14] Ibid., p. 172.

[15] E. Barber, 'The Bourgeoisie in 18th Century France'.

[16] A Ph.D. thesis by John Blacker (in the Senate House Library, London University) throws some doubt on this analysis by Barber.

[17] Quoted by Philip Abrams in S. Hutington (ed.), *Changing Patterns of Military Politics*.

[18] Sir John Smyth, *Sandhurst*, p. 263.

EDWARD JENNER:
THE HISTORY OF A MEDICAL MYTH

by

P. E. RAZZELL

EDITOR'S NOTE: The provocative title of Mr. Razzell's article will doubtless shock many readers, but it is the duty of the historian to take nothing for granted and to put to the question periodically the major assumptions of history, just as it is an editor's duty to give space to iconclasts as well as to idolists. The following article is frankly controversial and the editor considered its implications so important, both for medical history and current practice, that he has invited Professor A. W. Downie, M.D., F.R.S., of Liverpool University, an acknowledged authority in this field, to comment on Mr. Razzell's arguments. The latter has claimed the right to reply to Professor Downie's criticisms and both comment and reply will be found at the end of the article. Discussion is now open to readers and any further discussion, by Professor Downie or others, will be published in forthcoming issues of *Medical History*. The editor confines himself to remarking that the October issue will contain an interesting account of smallpox in Ethiopia which may be read as an implicit refutation of Mr. Razzell's case. Despite the long-continued use of inoculation in this close community, epidemics of smallpox raged until Jennerian vaccination was introduced in the nineteenth century. If Mr. Razzell's article and the ensuing debate prove nothing else we are given a lively demonstration that medical history is by no means a dead subject but is concerned with issues which are very much alive.

F.N.L.P

THE purpose of this paper is to demonstrate that vaccination is a more attenuated form of the eighteenth century practice of inoculation.[1] In a paper on eighteenth century population change,[2] I have argued that inoculation was effective in gradually eliminating natural smallpox, well before the advent of vaccination at the beginning of the nineteenth century. It is impossible to present the full evidence for this conclusion here, but only to illustrate it with selected statistics.[3]

Table 1

Smallpox Mortality from Epidemics in Boston, Mass., U.S.A. in the 18th century[4]

	1677–78	1702	1721	1730	1752	1764	1776	1788	1792
Population	4,000	6,750	10,700	13,500	15,684	15,500			19,300
Natural Smallpox									
Cases			5,759	3,600	5,545	699	304	122	232
Deaths	700	213	842	500	539	124	29	40	69
Deaths per 1,000 cases			146	139	97	177	95	328	298
Inoculated Smallpox									
Cases			287	400	2,124	4,977	4,988	2,121	9,152
Deaths			6	12	30	46	28	19	179
Deaths per 1,000 cases			21	30	14	9	6	9	20
Total Smallpox									
Deaths	700	213	848	512	569	170	57	59	284
Deaths per 1,000 cases population	175	32	79	37	36	11	10	6	10
Left the town					1,843	1,537			262
Escaped disease in town					174	519			221
Had Smallpox before					5,998	8,200			10,300

[1] Throughout this paper inoculation is used to mean variolation (except where stated), as this was the term used by eighteenth century contemporaries, some of whose writings we shall be considering.

[2] To be published in the *Economic History Review*.

[3] None of the figures in this paper ought to be taken too literally, as there are many problems with

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Three important conclusions are to be derived from this table: (1) The smallpox death rate was reduced from 175 smallpox deaths per 1,000 living in 1677–8 to ten per 1,000 by 1792, (2) this was achieved in spite of an increase in the virulence of the disease, (3) the reduced mortality may be directly attributed to inoculation, which protected the vast majority of the vulnerable population by the end of the eighteenth century.

An example of the effects of inoculation on smallpox mortality in England is to be found in eighteenth century Maidstone.

Table 2. Smallpox Mortality at Maidstone, Kent, 1752–1801[5]

Period	*Smallpox burials*	*All burials*
1752–61	252	1703
1762–71	76	1426
1772–81	60	1549
1782–91	91	1676
1792–1801	2	2068

A mass inoculation was conducted by Daniel Sutton in 1766 and its effects were described by Howlett in 1782:

> Upon casting an eye over the annual list of burials we see, that, before the modern improved method of inoculation was introduced, every five or six years the average number was almost doubled; and it was found upon enquiry, that at such intervals nearby the smallpox used to repeat its dreadful periodical visits . . . in the short space of thirty years it deprived the town of between 500 and 600 of its inhabitants; whereas in the fifteen or sixteen years that have elapsed since that general inoculation it has occasioned the deaths of only about sixty.[6]

The main reason why most historians thought that inoculation had been ineffective against smallpox was the set of smallpox mortality statistics for London. These were faulty in several ways,[7] but must be reinterpreted in the light of the fact that inoculation was utilized on a large scale much later in London than in the rest of the country, especially outside large towns.[8] Howlett stated this quite explicitly in 1781:

regard to classification of disease, etc. However, smallpox is a sufficiently distinctive disease to enable us to use these figures as indicators of trends.

[4] J. Blake, *Public Health in the Town of Boston (Mass.), 1630–1822*, Cambridge, U.S.A., 1959, p. 244. Royal Commission on Vaccination, 6th Report, *Parl. Papers 1896/47*, p. 762. H. R. Viets (Ed), *A Brief Rule to Guide the Common People of New England*, Baltimore, 1937, p. 35. The figures in this table do not balance, as some people inoculated were not inhabitants of the town, and were therefore not included in the total population.

[5] Figures compiled from the *Maidstone Parish Register*.

[6] J. Howlett, *Observations on the Increased Population . . . of Maidstone*, Maidstone, 1782, p. 8.

[7] For example no account is taken of the increased number of births.

[8] The reasons for this are complex and it is not possible to analyse them here.

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It may be thought, at first sight, that the healthiness of London is more increased than that of country towns. . . . But it must be remembered that the diminished mortality in the latter appears to be chiefly owing to the salutary practice of inoculation, whereas in the former, for want of universality, it has hitherto been of little advantage. . . . In provincial towns and villages, so soon as this disease (smallpox) makes its appearance, inoculation takes place amongst all ranks of people; the rich and the poor, from either choice or necessity, almost instantly have recourse to it; and where 200 or 300 used to be carried to their graves in the course of a few months, there are now perhaps not above twenty or thirty.[9]

It is in the light of these findings that we must re-examine the relationship between inoculation and vaccination. One aspect of the conventional medical view of the relationship is that inoculation differs from vaccination inasmuch as it gives rise to pustular eruptions other than at the site of injection and is consequently a source of infection to an unprotected population.[10] There is contemporary eighteenth century evidence to suggest, however, that this is not the case. None of the hundreds of incumbents making returns in the *Statistical Account of Scotland* at the end of the eighteenth century, specifies a case of inoculation spreading smallpox, which certainly would have happened had inoculation been infectious because of the partial inoculations of the gentry and farmers in some parishes.[11]

According to a letter sent from the Council of Geneva in 1791:

An epidemic of smallpox is of almost regular occurrence every five years, and between the epidemics it frequently happens that we have no natural smallpox whatever, little in the city or its vicinity. Inoculation began to be practised here in 1751, since which date we have inoculated a very large number of children annually, and with such marked success that the deaths have not exceeded 1 in 300. Although we have often had to inoculate with pus brought from a distance at times when there was no smallpox to be found in the city, and although children so inoculated have gone freely into the streets, walks, and other public places, before, during, and after the eruption, we have never observed that they were sources of contagion, nor that they produced any intermediate epidemics, nor that they accelerated the return of the periodical epidemic.[12]

An almost identical description was sent from the Hague:

The 200 persons who were inoculated at the Hague, about the end of the year 1768, without much regard to themselves or others, frequented all places of public resort; notwithstanding which no epidemic was produced, nor in the whole year did more than eight persons die of the smallpox, and of these three died in the spring, one by inoculation, and two by the natural disease, which they had caught at some other place and carried with them to the Hague, and the remaining five died towards the end of the year.[13]

There were similar experiences noted at Chester[14] and at Ware, Herts.,[15]

[9] J. Howlett, *An Examination of Dr. Price's Essay on the Population of England and Wales*, Maidstone, 1781, p. 94.
[10] The technical view of the relationship is that inoculation uses smallpox virus, whereas vaccination uses cowpox virus.
[11] See Sir J. Sinclair (Ed.), *The Statistical Account of Scotland*, 21 vols., Edinburgh, 1791–99.
[12] J. Haygarth, *A Sketch of a Plan to Exterminate the Casual Smallpox*, 1793, pp. 472–5.
[13] *Gentleman's Magazine*, *47*, 1777, p. 224.
[14] J. Haygarth, *An Inquiry how to Prevent the Smallpox etc.*, Chester, 1785, p. 588.
[15] J. C. Lettsom, *A Letter . . . upon General Inoculation*, 1778, p. 11.

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and many inoculators were well aware that their patients were not a source of contagion.

The most convincing evidence of the non-infectious nature of inoculation is provided by a series of experiments conducted during the late eighteenth century by Dr. O'Ryan, Professor of Medicine at the College of Lyons, France, part of which he described as follows:

> I placed a person in the eruptive fever of the smallpox by inoculation at the distance of about half a yard from four children properly prepared; each exposure continued one hour, and was repeated daily for a fortnight, reckoning from the commencement of the fever till the pustules were become perfectly dry: not one of the four received the infection. Two months afterwards, I inoculated three of these children; they had the distemper in a very mild manner and recovered without difficulty.[16]

O'Ryan concluded from his experiments

> that there is no risk of contracting it [smallpox], provided the person who is liable to the infection, keeps himself at a very little distance from patients in the smallpox, or from things which they have touched.[17]

Although we now know this view to be erroneous, we must still explain the results of his experiments.

A clue to the answer to our problem is to be found in Dixon's recent text on smallpox. In discussing the infectivity of scab virus he writes: '. . . in practise scab virus seems to lack epidemic potential. I have suggested (Dixon 1948) that the virus extruded through the skin, perhaps modified by its passage, is in some way different from the virus from the respiratory tract.'[18] Logically, the opposite also applies, i.e., the virus injected through the skin is also modified in some fundamental way. Therefore an inoculated person, whatever number of pustules erupt, would not be infectious, as all the smallpox viruses in his body would have derived from a stock of modified viruses which had been extruded through the skin of another person's body (the person from whom the virus was originally taken) and then passed through his own skin.

As the degree of infectivity of smallpox is probably connected with the degree of severity of the disease,[19] we would expect the transmission of the virus through the skin to produce milder types of the disease. This is in fact what happens, as all the inoculators well knew. Mowbray, Gatti and the Suttons all produced much milder and safer results from inoculation by arm-to-arm transmission. Gatti ran into difficulty over his inoculation in 1765 of the Duchess de Bouffle, who had no pustular eruption except at the site of inoculation and suffered an attack of natural smallpox two and a half years later,[20] a problem which would occupy the vaccinators forty years later. Gatti appears to have achieved these very mild results by taking the smallpox virus for his inoculations from the site of a previous inoculation, rather than from one of the pustular eruptions around the body.[21]

[16] J. Haygarth, *A Sketch of a Plan. . . .*, pp. 82, 83.
[17] *Ibid.*, pp. 78, 79.
[18] C. W. Dixon, *Smallpox*, 1962, p. 298.
[19] *Ibid.*, p. 298.
[20] C. Creighton, *The History of Epidemics*, 1894, vol. II, pp. 495, 496.
[21] A. Gatti, *New Observations on Inoculation*, 1768.

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Fortunately, we have some experimental evidence on the degree to which smallpox virus can be attenuated. In 1777 John Mudge, a Plymouth surgeon, reported the following experiment:

> Messrs. Longworthy and Arscott, surgeons, in the spring of 1776, inoculated at Plymton, a neighbouring town, forty patients; of which number, thirty were injected with crude matter from the arm of a young woman, five days after she herself had been inoculated (i.e. from the site of inoculation before the eruption of pustules) with concocted matter (from a pustule around the body), which eventually did produce in her a pretty smart fever, and a sufficient number of eruptions. The other ten were inoculated with matter of another kind, which I procured, in a concocted state, from a pustule of the natural smallpox. The arm of all the forty patients took the injection; and the latter ten, after the eruptive fever, had the smallpox in the usual way. Of the other thirty, though the injection took place on their arms, so as to inflame them considerably, and to produce a very large prominent pustule, with matter on it, on each of them, yet not one of them had any eruptive fever, or a single subsequent eruption, on any part of the body. . . . It is to be remarked too that the matter which was in those pustules having been used to inoculate others produced on them exactly the same appearances, unattended also with either fever or smallpox.[22]

In other words it was possible to attenuate the smallpox virus to such an extent that only a single pustule was produced at the site of inoculation and this was achieved by taking the virus from the site of a previous inoculation. Adams repeated the same experiment at the beginning of the nineteenth century and was able to produce a whole series of cases in which there was only an eruption at the site of inoculation. *He compared the latter with typical vaccine vesicles and claimed that they were identical.*[23] This was a conclusion confirmed by Guillou, who was also able to produce a typical vaccine vesicle at the site of inoculation.[24] Dr. John Walker, Director of the Royal Jennerian Society, wrote to Lettsom in 1813:

> I have, from the first introduction of vaccination, after having observed its symptoms and progress, entertained an opinion respecting its native difference from those who suppose it a substitute only for the (inoculated) smallpox. . . . *Now I have from an early part of my practice, been in the habit of diluting the smallpox virus with water, previous to its introduction into the system* [author's italics;] and in every instance I have then always found the disease mild, and the fever slight: this led me to the conclusion above hinted at. . . . I believe the variola and vaccine (so called) to be, at bottom, the same disease, and could wish that the term variola mitior were employed instead.[25]

As Creighton observed, 'the very Director of the Jennerian Institute was among the prophets of the old inoculation'.[26] However, from our present point of view what is significant is that Walker was able to produce the single local vesicle typical of vaccination, through a process of attenuating smallpox virus.

It is in the light of these neglected facts that we must reinterpret the history of vaccination itself. After a few initial experiments with cowpox in 1796 and

[22] J. Mudge, *A Dissertation on the Inoculated Smallpox*, 1777, pp. 20–22.
[23] See Royal Commission on Vaccination, 4th Report, *Parl. Papers*, *1890–91/44*, p. 52.
[24] *Ibid.*, p. 53.
[25] T. J. Pettigrew, *Memoirs of the . . . late John Coakley Lettsom*, 1817, vol. 3, pp. 350, 351.
[26] Creighton, *op cit.*, p. 590.

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1798, Jenner's original vaccine lymph was lost, and it was not until the end of January 1799, when cowpox was discovered in Gray's Inn Lane by Woodville, that experiments were resumed. Woodville immediately sent Jenner some lymph to check its suitability. With this lymph, Jenner operated on twenty persons and reported to Woodville: 'Berkely, February 1799. The rise, progress, and termination of the pustules created by the virus were exactly that of the true cowpox.'[27] Woodville was completely confused about the relationship between vaccination and inoculation, and later wrote: 'The virus which Dr. Jenner declared to be perfectly pure and genuine was taken from the arm of a (smallpox) hospital patient who had 310 pustules, all of which suppurated.'[28] Woodville, who was a doctor at the London Smallpox Hospital, had found that a majority of 500 people vaccinated by him had pustular eruptions similar to those that took place during inoculation. The conventional medical explanation of this is that repeated recently by Dixon:

> Unfortunately Woodville vaccinated his cases at the Smallpox Hospital, and at least two-thirds of them showed some general eruptions. It is almost certain, that under these circumstances the patients were either inoculated (injected) with a mixture of vaccine and variola virus from contaminated lancets, were vaccinated and naturally infected with smallpox at the same time, or, in some cases, were vaccinated and then variolated from three to five days later, when they again had a double infection.[29]

This interpretation neglects a considerable body of evidence to the contrary, particularly that supplied by Jenner himself. At the beginning of 1800 he wrote a letter to Lord Egremont, one of his patrons, who had complained that some of the vaccine sent from London had produced pustular eruptions when used on his family at Petworth. Jenner wrote:

> In many places where the (vaccine) threads were sent, a disease like a mild smallpox frequently appeared; yet, curious to relate, the matter, after it had been used six or seven months, gave up the variolous character entirely, and assumed the vaccine; the pustules declined more and more, and at length became extinct. I made some experiments myself with this matter, and saw a few pustules on my first patients; but in my subsequent inoculations (vaccinations) there were none.[30]

It is quite clear from this letter that the conventional medical explanation (e.g. Dixon's) of the pustular eruptions in Woodville's cases of vaccination is incorrect, for pustular eruptions occurred outside the London Smallpox Hospital where contaminated lancets, mixed injections or natural smallpox cannot be invoked as explanations (this is particularly true of Jenner's own cases). These pustular eruptions gradually disappeared as the new vaccine was transmitted from arm-to-arm, using the site of a previous inoculation. Thus the so-called vaccine was none other than smallpox virus, which was attenuated

[27] William White, *The Story of a Great Delusion*, 1885, p. 147.
[28] *Ibid.*, p. 149.
[29] Dixon, *op. cit.*, pp. 119, 120.
[30] J. Baron, *Life of Dr. Edward Jenner*, 1827, vol. 1, pp. 314, 342.

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in a manner already familiar to some of the inoculators. The vaccinators were producing results similar to those produced by Arscott and Longworthy, Gatti, Adams and Walker, through taking smallpox virus from sites of previous inoculations and transmitting it from arm-to-arm; the only difference being that they thought that they had discovered a new process. Arscott, Longworthy and Mudge had rejected this attenuated technique which produced only a local pustule, as they felt it would give insufficient protection against future attacks of smallpox (in this they were right) and it was left to the vaccinators to utilize unknowingly the same technique twenty-four years later.

However, we must still try to explain what the relationship is between the cowpox and smallpox viruses. Unfortunately, the virologists do not seem to be in a position to settle this problem and it is not even agreed whether the one virus is autonomous of the other.[31] According to one authority:

> At the present day the general opinion agrees with that held by Jenner, that cowpox is simply smallpox much modified by passage through the cow. It might be supposed that this fact would be one easy of demonstration, and cows have by many observers, e.g. Woodville in 1799, by Ceely, by Badcock, and by Thiele of Kazan in 1838, been experimentally inoculated with smallpox but in most cases the disease, when thus artificially produced in cows, appears to retain a considerable degree of virulence, and to produce general though slight symptoms when again communicated to human beings, instead of the purely local symptoms of ordinary vaccination.[32]

Copeman attempted to explore the relationship between smallpox and cowpox experimentally:

> He first inoculated a monkey with smallpox virus and then inoculated a calf from such an infected monkey. This resulted in typical vaccine, from which good strains of vaccine lymph were obtained. On the basis of this experience Copeman suggested that cowpox may actually have originated in the eighteenth century from inoculated smallpox, as the local sore produced by the inoculated incision frequently was very itchy, and milkers who scratched their arms may easily have conveyed infectious matter to the cow's udder.[33]

From our point of view the transmission of the smallpox virus through a cow or any other non-human animal, is an irrelevance, inasmuch as smallpox inoculation can be attenuated into vaccination merely by arm-to-arm transmission of the virus, using the previous sites of inoculation. This hypothesis is the only one to explain the manifold contradictions contained in all the evidence.[34]

Does this conclusion mean that the reputation of Jenner is undeserved? He, who had been inoculated in the old method as a boy during the mass inoculation at Wootton-under-Edge in 1756, was an inoculator using the Suttonian method before he claimed to have discovered vaccination.[35] The only advantage

[31] Dixon, *op. cit.*, pp. 119, 120, 163.
[32] W. A. R. Thomson, *Black's Medical Dictionary*, 1963, p. 942.
[33] G. Miller, *The Adoption of Inoculation . . . in England and France*, Philadelphia, 1957, pp. 19, 20.
[34] This includes the so-called phenomena of generalized vaccinia, which on the present hypothesis is nothing other than what eighteenth century contemporaries would have considered a typical inoculation. It would also explain why 'although vaccinia and cowpox have common features of wide host range, serologically variola is more closely related to vaccinia'. Dixon, *op. cit.*, p. 163.
[35] J. J. Abraham, *Lettsom*, 1933, p. 192.

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the latter had over the more traditional methods of inoculation was that it appeared to cause fewer direct deaths. The problem in evaluating this claim is that many deaths were attributed to inoculation, which were probably due to the fact that many people had caught smallpox before being inoculated. Thus for example, in Boston, Mass., inoculation was forbidden by law and was only allowed when the presence of an epidemic created such panic as to make it inevitable. As several thousand people were inoculated, some of them would have caught smallpox before being inoculated, and their subsequent deaths would be incorrectly attributed to the inoculation. In more controlled conditions the death rate from the mild Suttonian method of inoculation was virtually nil. The Sutton's claimed in 1768 'that about 55,000 had been inoculated by them since the year 1760, of which number, six only had died'.[36] Among the 5,694 people inoculated at the London Smallpox Hospital during the years 1797–9 there were only nine deaths. By the beginning of the nineteenth century the inoculators had attenuated their viruses sufficiently to be able to eliminate the risk of death altogether; for example Dr. Forbes, a supporter of vaccination and an opponent of inoculation, had to report that of the 2,500 people inoculated in the Chichester area in 1821 not one died.[37] Inoculation had the advantage over the more attenuated vaccination of conferring a much longer period of immunity against future attacks of smallpox, and this was of course because of the larger numbers of antibodies produced. This much greater period of immunity was no mean advantage at a time when smallpox was such a constant threat.

Generally we must conclude that Edward Jenner's contribution to the history of medical innovation has been greatly over-estimated, and at most he was one of many innovators in the technique of inoculation against smallpox.

Comment by Professor A. W. Downie, M.D., F.R.S.

I have read through this paper carefully and it appears to me that the author has been very selective in quoting sources to uphold his thesis.

In his general proposition that the reduction in the smallpox death rate between 1677 and 1792 was due to smallpox inoculation, he has ignored the importance of other factors. It is true that the figures from Boston (Table 1) would appear to lend some support to his thesis, but he ignores the fact that in Boston very strict quarantine regulations were enforced to prevent the introduction of smallpox into that City. Isolation of cases when they occurred was strictly enforced. This and the quarantine regulations introduced to prevent the importation of smallpox into the town, were probably more important measures than inoculation in determining the diminution in incidence of the disease over the period covered in Table 1.

The author appears to believe that by the end of the eighteenth century

[36] R. Houlton, *Indisputable Facts, Relative to the Suttonian Art of Inoculation*, Dublin, 1768, p. 10.
[37] Dr. J. Forbes, 'Some account of the small-pox lately prevalent in Chichester and its vicinity', *London Medical Repository*, Sept. 1822, pp. 211–15.

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inoculation of smallpox was very widely and generally applied. This would seem very far from being the case. (Up to 1764 only 5,554 persons in the whole of Scotland had been inoculated with smallpox according to Alexander Monro Senior.) It is obvious from Haygarth's correspondence published in his *Sketch* (1793) and in the letter to Percival of Manchester, that after the first few years of the introduction of inoculation against smallpox in Chester, the poor people in the town would not avail themselves of this measure. Indeed, he regrets that no-one had come forward at all for inoculation and that the poor preferred to acquire the disease in the natural way. With reference to the diminution of smallpox as a result of inoculation towards the end of the eighteenth century, he states that in 1774 only 1/14th of the population of Chester had *not* suffered from the disease. This was at the time when inoculation of the smallpox was not available to the poorer people in the town. Similar observations were made in Leeds and Newcastle. So much for the author's suggestions that inoculation had greatly lessened the ravages of smallpox by the end of the eighteenth century! It seems much more likely that the diminished mortality from the disease at this time was due to the recognition of the infectious nature of the disease and measures of isolation being introduced to prevent its spread, such as the provision of isolation wards in hospitals and isolation of patients at home, together with improved housing and nutrition of the poor.

The author also quotes reports apparently showing that the inoculated disease was not infectious. This, however, is not supported by evidence from other sources. Maitland recorded in 1722 that the little girl, Mary Batt aged two years, who was inoculated by himself, infected six domestic servants with typical smallpox from which one of them died. It is also apparent from Haygarth's *Sketch* (1793) that the disease was frequently spread from inoculated to susceptible persons. Indeed, it was an essential part of Haygarth's plan that those inoculated with smallpox must isolate themselves at home to avoid spreading infection. He quotes several instances where such spread did in fact occur. The author of the manuscript has made selections from the letters published in Vol. II of the *Sketch*, choosing those purporting to show that the inoculated disease was not infectious. He has ignored other letters in the same volume which provide evidence of spread of infection from inoculated persons. He also ignores the fact that even the casual smallpox is not as highly infectious as many people think—a point also stressed in all Haygarth's writings.

There is evidence from the observations of the Suttons, Dimsdale and others, that the introduction of the Suttonian technique, of taking material from the site of inoculation of the smallpox after four or five days for further inoculations, produced a milder type of inoculation smallpox than had previous practice. When this technique was followed the mortality from the inoculated disease became much less than in the earlier years (1721–30), but even at the end of the eighteenth century most authorities agreed that the mortality from inoculated smallpox was still of the order of 1/200 to 1/500.

The author's main contention that the vaccinia virus now employed has been derived from smallpox virus attenuated by repeated passage through arm

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vaccination, may be true, but proof of this is not available at the present time. The strains at present in use for vaccination have been so long passed in laboratory animals that the history of their origin is uncertain. It has, however, I think been established from Jenner's experiments and those carried out with fresh stocks of cowpox in 1799, that cowpox infection did protect against smallpox. It is true, as Dixon has maintained, that Woodville's experiments were unreliable in that his inoculations of cowpox were carried out in a smallpox hospital and many of the subjects were subsequently tested by variolation a few days later. These two facts made it very difficult to be sure that Woodville's observations had much bearing on the value of cowpox virus as an immunizing agent. It is, however, also clear from the observations of Ceely, published from 1839 onwards, that inoculation of genuine cowpox virus would protect against smallpox. Ceely gave very clear descriptions of the effects of inoculating cowpox virus on humans and, indeed, isolated fresh stocks of virus from the natural disease in cows or persons infected from them. In my opinion, Dixon's comments on Woodville's work are quite justified.

The author mentions the adaptation of smallpox virus to propagation in the cow. Many such observations of this kind were recorded in the nineteenth century but they are all of doubtful value because cowpox was sometimes inoculated on the same animals and the later experiments were carried out (e.g. Copeman's) with variola virus in institutes where strains of vaccinia were also in use. French workers showed many years ago that vaccinia virus spread very readily amongst cows and suggested that many of the reported successful inoculations with variola in cows were, in fact, cross infection of the animals with strains of vaccinia in use in the same establishments. All recent attempts (in the last twenty-five years) to infect cows with smallpox virus and to pass the virus to successive animals, have failed, even when the monkey has been used as an intermediate host. (Our own attempts to convert variola to vaccinia by inoculation of animals have been completely unsuccessful.) (See also Herrlich *et al. Arch. ges. Virusforschung*, 1963, *12*, 579.)

We have no doubt that cowpox is a natural disease of cattle and is not derived from variola. We have isolated at least a dozen strains of cowpox virus from the natural disease of cattle or from lesions on the hands of those working with infected animals. All these strains of virus are quite different from strains of variola virus and also from current strains of vaccinia virus. However, these strains of cowpox virus are immunologically practically identical with vaccinia virus and with variola virus. Immunisation of animals with cowpox virus produces antibody which is apparently effective against variola and vaccinia viruses. Our cowpox strains have the same host range as vaccinia strains and can be readily passed on to a variety of laboratory animals. This feature is not shown by a number of strains of variola virus which we have tested in this way.

I apologize for writing to you at such length, but I cannot agree with much of the argument in the enclosed manuscript. The author has selected to support his thesis only such evidence as would suit his purposes and has neglected many other works which would appear to refute his arguments.

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Mr. Razzell's reply

I will deal with Professor Downie's points in the order that they were raised. He writes: 'This (the isolation of smallpox cases) and the quarantine regulations taken to prevent the importation of smallpox into the town, were probably more important measures than inoculation in determining the diminution in incidence of the disease over the period covered by Table 1.' Yet if you look at Table 1 you will see that the numbers escaping the disease in and out of town amount to only *483* people out of a total population of *19,300* in 1792. Table 1 *indisputably* demonstrates that the diminution in the number of smallpox deaths may be directly attributed to the effects of inoculation.

The second point raised concerns evidence for the hypothesis that inoculation did spread natural smallpox to an unprotected population. Maitland's example of an inoculated two-year-old girl spreading the disease to six domestic servants is cited. This incident occurred in 1722 when the English inoculators invariably used natural cases of smallpox as the source of their virus. As Dixon has written:

> In spite of the warnings in the earlier writings of the desirability of sending someone else to collect the smallpox matter so as to avoid injecting the inoculated person simultaneously with the natural disease (from respiratory virus on clothing, or in other ways from an infectious patient), it seems clear that Armyand as well as Maitland did not realize the effect of inoculating simultaneously with, or after contact with, natural smallpox in confusing the statistics of inoculation.[38]

The standard practice of later inoculators was to take smallpox matter from previously inoculated cases or to carry it with them dried on threads, thus avoiding the problem of transmitting the infection from natural cases. Even if we reject Dixon's point, Maitland's example of the danger of inoculation is very suspect, because many cases of natural smallpox were to be found in London every week of every year during this period (see the London Bills of Mortality)—therefore it is quite possible for the domestic servants to have caught the disease naturally from another source. A much better type of evidence is that referring to a situation where a partial inoculation takes place in an isolated rural area in response to the threat of an epidemic. In the twenty-one volumes of the *Statistical Account of Scotland* many of the incumbents described the recent history of diseases in their parishes—of the 234 incumbents who mentioned that inoculation has taken place in their parishes *not one* specified an instance of it spreading the natural disease to vulnerable members of the population. An even more convincing example of this point is supplied by Dr. John Forbes (a supporter of vaccination and an opponent of inoculation), who in his description of the smallpox epidemic of 1821 in the Chichester area had to admit that

> during the winter months he (a local inoculator) inoculated upwards of 1,000 persons (around the country area outside Chichester) . . . not more than 130 or 140 cases of natural smallpox

[38] C. W. Dixon, *Smallpox*, 1962, p. 232.

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were witnessed by all the surgeons during the course of the epidemic. Of these, by far the greater number occurred *in* Chichester, owing to the continued resistance of the surgeons to inoculate.[39]

Professor Downie goes on to point out Haygarth's belief in the contagiousness of inoculation. All contemporaries believed that inoculation spread smallpox, inasmuch as they believed it to be itself a mild form of natural smallpox. However, when it came to a question of empirical evidence rather than theoretical belief, there is no doubt about the conclusion to be drawn. Haygarth himself concluded in 1781 from his experience in Chester that

Inoculation did not, as some might apprehend, spread the contagion, but appeared to produce a quite contrary effect. For in the districts where most patients were inoculated, there remained the fewest in the natural smallpox, and, in the districts where the smallest number were inoculated, the distemper was afterwards most general.[40]

The most conclusive evidence for the non-contagious nature of inoculation is the series of experiments by O'Ryan which were quoted in the text of my paper, as well as the fact that the early 'vaccines' were directly derived from smallpox viruses without transmission through a cow (e.g. Walker's 'vaccine') and yet nobody has ever suggested that such 'vaccines' spread natural smallpox.

According to Professor Downie the Suttonian technique consisted 'of taking material from the site of inoculation of the smallpox after four or five days for further inoculation'. This was not the case—the Suttons took their material from any pustule around the body and not just from the site of inoculation; they also took their material from pustules at every stage of development.[41]

The essence of their technique was the use of a lancet, making the lightest of scratches and inserting the minimal amount of material. As for the mortality from inoculation, it is very difficult to assess independently of mortality due to natural smallpox before inoculation had time to take effect. As I have already indicated, in the controlled conditions of the London Smallpox Hospital its mortality was negligible, particularly in the later period—e.g. of the 431 in-patients inoculated between 1808 and 1813 not one died.[42] Pearson, one of Jenner's chief supporters, conceded that the mortality from inoculation was neligible and quoted two examples:

Dr. William Heberden informs me, that at Hungerford, a few years ago, in the month of October, 800 poor persons were inoculated for the smallpox, without a single case of death. No exclusion was made on account of age, health, or any other circumstances, but pregnancy; one patient was eighty years of age; and many were at the breast, and in the state of toothing. Dr. Woodville acquaints me, that in the current year (1798), from January to August inclusive, out of 1,700 patients inoculated at the Inoculation Hospital, including the *in* and *out* patients, *only two died*, both of whom were of the latter description.[43]

[39] Forbes, *op. cit.*, pp. 213, 215.
[40] Haygarth, *An Inquiry. . . .*, p. 188.
[41] R. Houlton, *Indisputable Facts Relative to the Suttonian Art of Inoculation*, Dublin, 1768, p. 40.
[42] J. R. Hutchinson, 'A historical note on the prevention of smallpox in England', *Health Annual Reports* (Ministry of Health), 1945–46. Appendix A, p. 122.
[43] G. Pearson, *An Inquiry Concerning the History of the Cow Pox*, 1798, p. 79.

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It should also be remembered that Walker's 'vaccine' which was the one most widely used in early nineteenth century England, was in fact diluted and attenuated smallpox virus—and it gave rise to a negligible rate of mortality.

I have not disputed the power of cowpox to protect against smallpox, but have argued that vaccinia was directly derived from smallpox. Professor Downie counters this point by stating that it has been impossible during the past twenty-five years to infect cows with smallpox virus, i.e. produce cowpox from smallpox. He suggests that the very many previous successes in doing this were due to 'cross infection of the animals with strains of vaccinia in use in the same establishments'. This argument is implausible in the light of the fact that the purpose of trying to infect cows with smallpox was not experimental, but was an attempt to produce vaccinia which was otherwise not available. Vaccines were difficult to maintain and acquire, hence the attempts to produce them 'artificially'.[44] This being so it is highly unlikely that vaccinia was present in these establishments. If 'cowpox is a natural disease of cattle' why is it not to be found in New Zealand where there is little or no smallpox and vaccination, and why do not cases of human cowpox arise in slaughterhouse workers? As Dixon has said: 'This would suggest that cowpox is not a natural disease of bovines.'[45] Cowpox appears to have increased considerably with the advent of inoculation in the eighteenth century and declined during the nineteenth and twentieth centuries when inoculation disappeared and the amount of vaccination diminished. This would suggest that Copeman was right in thinking that for smallpox to be suitable for adaptation to the cow it must be taken from an inoculated rather than a natural case (it should be noted that there were several mass inoculations in Gloucestershire at about the time that Jenner discovered his first cases of cowpox, e.g. in 1795 at Berkeley and at Dursley in 1797 when over 1,100 people were inoculated). However, for the purposes of my paper it is not necessary to demonstrate that cowpox derives from smallpox, but merely to show that the early 'vaccines' were directly derived from smallpox without using an intermediary host such as the cow, and that this is indisputable is demonstrated by the fact that Walker admitted it to be so.

As for Professor Downie's last point about inoculation not being very widespread at the end of the eighteenth century, I have dealt very fully with this question in the paper to be published in the *Economic History Review*. In fact the best evidence is to be found in the writings of Jenner and his early supporters, e.g. Jenner wrote: '. . . the common people were rarely inoculated for the small-pox, till that practice was rendered general by the improved method introduced by the Suttons. . . .'[46] These early writings are full of references to mass inoculations, and most of Jenner's cases of people with natural cowpox had been inoculated at some time during their lives. Professor Downie takes the experience of the towns as typical for the country as a whole, but only a small minority of the total population lived in such towns. In a country village

[44] See for example J. Jones, *Vaccination*, Louisiana, Baton Rouge, 1884, pp. 401–3.
[45] Dixon, *op. cit.*, p. 162.
[46] *The Medical Repository*, New York, 1802, vol. 5, p. 239.

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or market town epidemics of smallpox were very infrequent, sometimes occurring only every twenty or thirty years. When such an epidemic did occur it struck such a large proportion of the total population (children and adults) and was so virulent (lack of a pool of antibodies) that the resulting panic drove everyone to be inoculated, e.g. when an epidemic broke out in Blandford, Dorset, in 1766 'a perfect rage for inoculation seized the whole town'.[47] In a place like Chester only a fourteenth of the population (all infants) had not suffered from smallpox, because it was virtually endemic, i.e. in the town nearly every year. This bred a fatalistic attitude amongst the parents of poor children, particularly as the piecemeal nature of smallpox mortality did not lead to a spectacular demonstration of the effects of inoculation as it did in the country areas. Inoculation was virtually universal in such areas by the end of the eighteenth century and was also making rapid headway in the large towns by that time.

It is true that only 5,553 persons were reported to have been inoculated in Scotland by 1764, but inoculation made much greater progress in England than Scotland (200,000 inoculations were reported to have taken place in England by 1766). The great watershed in the history of inoculation was the popularization of the Suttonian technique from about 1768 onwards; the Suttons alone inoculated 245,000 people in the nine years between 1768 and 1777. Although the spread of inoculation continued to lag in Scotland in comparison with England, of the 243 incumbents who discussed it in the Statistical Account of the 1790s, 162 said that it was widely practised in their parishes,[48] as against ninety-one who said that it had still to become general.

Perhaps what we now know about inoculation could be put to some practical use. Many tens of thousands of people die of smallpox each year in the world. This is partly due to inadequate medical facilities and the difficulty of obtaining vaccine in very isolated, primitive areas. It is just in such areas that inoculation would be most useful. Rosenwald has recently described mass inoculations amongst tribal Africans in Tanganyika—the operation appears to have been both safe and effective.[49] Undoubtedly some form of further observation or possibly even experimentation is necessary before smallpox inoculation can legitimately be used in certain special and limited circumstances.

[47] C. Creighton, *The History of Epidemics*, vol. II, 1894, p. 513.

[48] For example the incumbent of Duirinish, Skye, wrote in 1792: 'this increase in population may be attributed . . . above all, to the inoculation of the smallpox, which has been universally practised in this island for thirty years past, and has been the means of preserving many lives.'

[49] C. D. Rosenwald, 'Variolation', *Med. Offr.*, 1951, **85**, 87.

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Population Change in Eighteenth-Century England. A Reinterpretation

By P. E. RAZZELL

There have been two traditional explanations of the acceleration in population growth which occurred during the middle of the eighteenth century: (1) the Malthusian view that it was a consequence of the Industrial and Agricultural Revolutions through an improved standard of life; (2) that it was the result of various medical innovations independent of these Revolutions. The problem posed by these competing interpretations is central to English economic and social history: did the Industrial and Agricultural Revolutions create their own future labour force and expanding numbers of consumers, or were they themselves children of a Population Revolution which preceded them?

Economic historians have attempted to answer this question by estimating population, birth- and death-rates at decennial intervals throughout the eighteenth century. Professor Krause, however, has questioned the validity of the traditional method for the period before 1781 when aggregate statistics of Anglican baptisms and burials are available only for every tenth year from 1700 to 1780. He has pointed out that the use of one conventional assumption about English demographic data with reference to Sweden would exaggerate the amount of actual increase of population in that country between 1750 and 1780 by over 61 per cent.[1] Krause has attempted to use the statistics of annual baptisms and burials from 1780 onwards by making certain questionable assumptions about changes in the baptism/birth and burial/death ratios during the period 1781–1850. He concluded that a rise in the birth-rate rather than a fall in the death-rate was 'the major variable in English demography'.[2]

This has led the medical historians McKeown and Record to state that 'the data (on mortality and natality) are so treacherous that they can be interpreted to fit any hypothesis, and it seems preferable to rely on assessment of the sensitivity of the birth-rate and death-rate, and their relative effectiveness, in a period when both rates were high'.[3] This they had done in their own work and after reviewing the history of all the major diseases and preventive measures taken against them, concluded that the 'fall in the death-rate during the eighteenth and nineteenth centuries was not the result of medical treatment as Griffiths and others had supposed. Only in the case of vaccination against smallpox is there any clear evidence that specific therapy had a substantial

[1] J. T. Krause, 'Changes in English Fertility and Mortality, 1781–1850', *Economic History Review*, 2nd ser. XI (1958–59), p. 53.

[2] *Ibid.* p. 69.

[3] T. McKeown and R. G. Record, 'Reasons for the Decline of Mortality in England and Wales during the 19th Century', *Population Studies*, XVI (1962), pp. 94–95.

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effect on the prevention or cure of disease earlier than the twentieth century. The decline in mortality from diseases other than smallpox was due to improvement in living conditions, and to changes in virulence and resistance upon which human effort had no influence.'[1]

Krause, however, has pointed out that vaccination did not become really widespread until the 1840's and has argued that the average standard of living probably deteriorated slightly between 1780 and 1821 when population was increasing very rapidly.[2] Chambers, in his study of the Vale of Trent region, examined the relationship of food-supply to mortality-rates and concluded that population 'was vulnerable to disease, but not as a result of famine. Epidemics could do their own work without its aid, nor, it would seem, did they require the assistance of gin.'[3] A similar conclusion was reached by Pickard after analysing the relationship between food prices and changes in mortality and natality in eighteenth-century Exeter.[4] It should also be remembered that from 1838 to 1875, when the standard of living was undoubtedly rising rapidly, the overall death-rate was virtually constant.[5] It is in the light of all these contradictory facts that McKeown and Record have been reduced to making the following desperate statement: 'When we have eliminated the impossible (medical explanations of population growth), whatever remains (economic explanations), however improbable, must be the truth.'[6]

I

This paper is intended as a summary of research to date on the cause of the increase in population in eighteenth-century England.[7] Before discussing these causes it is necessary to estimate the size of population during the eighteenth and early nineteenth centuries, in order to appreciate the magnitude of change during this period. The estimates of population used in this paper are those derived from the returns of marriages made from several thousand parishes which were published by Rickman in 1841.[8] These estimates have several advantages: (1) unlike baptisms and burials, the overwhelming majority of dissenters' marriages took place in the Anglican church.[9] (2) The registration of marriage is generally considered to have been the most reliable.[10] (3) The

[1] T. McKeown and R. G. Brown, 'Medical Evidence Related to English Population Changes in the 18th Century', *Population Studies*, IX (1955), p. 139.

[2] Krause, *op. cit.* pp. 63–65.

[3] J. D. Chambers, 'The Vale of Trent, 1670–1800', *Econ. Hist. Rev.* Supplement 3, p. 29.

[4] R. Pickard, *Population and Epidemics of Exeter* (Exeter, 1947), p. 67.

[5] See B. R. Mitchell and P. Deane, *Abstract of British Historical Statistics* (Cambridge, 1962), pp. 36, 343–58.

[6] McKeown and Record, *op. cit.* pp. 94, 95.

[7] The paper is really a series of hypotheses illustrated occasionally by statistical and other evidence. It is hoped to incorporate detailed evidence into a monograph at a later date.

[8] Rickman's figures for marriages were generally derived from over 4,000 parish registers. See G. Talbot Griffiths, 'Rickman's Second Series of Eighteenth Century Population Figures', *Journal of the Royal Statistical Society*, 92 (1929), p. 263.

[9] The best confirmation of this is to be found in the *Report on Non-Parochial Registers*, P.P. 1837–38 (28) where it is seen that there were virtually no non-Anglican marriage registers kept for the eighteenth century.

[10] See J. C. Cox, *The Parish Register of England* (1910), p. 76. W. E. Tate, *The Parish Chest* (Cambridge, 1946), p. 65. G. Talbot Griffiths, *Population Problems of the Age of Malthus* (Cambridge, 1926), p. 33.

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estimates are based on three-year clusters of returns rather than single years, a procedure which is much more likely to reduce fluctuations of the marriage-rate from one time to another.[1] The basis of Rickman's own estimate was the assumption that the ratio of the number of marriages to total population in 1800, was the same for the periods 1699–1701 and 1749–51, i.e. that the marriage-rate was constant between 1700 and 1800. It is impossible to test this assumption in any detail, although there are a few scattered statistics available to suggest that it is not too unreasonable.

The Marriage-Rate throughout the Eighteenth Century

Place	Total population	Approximate period	Marriage-rate/1,000 population
7 Market towns [2]	27,043	1724–36	8·7
54 Villages [2]	19,607	1724–36	8·4
11 Towns [3]	37,541	1770's	8·5
England and Wales [4]	8,892,436	1795–1805	8·8

These figures must not be taken too literally as they refer to places of different sizes and locations; the figure for 1795–1805 is somewhat arbitrary because of the flaws in the registration of both marriages and population.

Be that as it may, the figures for marriage-rates indicate that there were no marked long-term changes in the marriage-rate throughout the eighteenth century. This conclusion is confirmed by at least one local study of population change during the same period.[5] The estimates of population size from the returns of the number of marriages are as follows: [6]

[1] An examination of the Swedish statistics for the eighteenth century, for example, shows that three-yearly clusters fluctuated far less than single years in terms of the marriage-rate. See *Historical Statistics of Sweden, 1720–1950* (Stockholm, 1955), pp. 39–41. The long-term marriage-rate in Sweden was remarkably stable between 1751 and 1825. See G. Sundbärg, *Sweden, Its People and Its Industry* (Stockholm, 1904), p. 96.

[2] Thomas Short, *New Observations On Bills of Mortality* (1751), p. 133.

[3] J. Howlett, *Observations On the Increased Population... Of Maidstone* (Maidstone, 1782), p. 82.

[4] I have excluded from this population figure the numbers in the army and navy; also I have not corrected for under-enumeration as a few marriages were also not registered because of the non-Anglican marriage of Quakers, Jews and Roman Catholics, as well as various illicit marriages in sea-ports and elsewhere. For the source of the population figure see *Census of Great Britain, 1851*, pp. xxiii, xxvi.

[5] Chambers, *op. cit.* pp. 54, 55.

[6] These estimates are re-computations of Rickman's figures. The following adjustments were made: (1) 5 per cent was added to the 1801 enumerated population because of estimated under-enumeration. See Krause, *op. cit.* p. 60. (2) Rickman took the number of marriages in the single year 1800 as the basis of his marriages/population ratio. This has been re-computed on the basis of the years 1800–02 so that the basic ratio is derived from a three-year cluster of marriages like all the previous periods. The original estimates are those Rickman arrived at by treating England and Wales as one unit and may be found in Griffiths's article in *Journal of the Royal Statistical Society*, 92 (1929), p. 263. See also J. Rickman, *Parishes Possessing Registers Extant 1570 and 1600 with their Population in 1801*, Document M. 74·10 in the General Register Office Library. (3) No allowance was made for the numbers in the armed service. The population figures are not intended as exact estimates, but rather as indications of the magnitude of change in the size of the population during the eighteenth and early nineteenth centuries. For the source of the 1801 and 1851 figures, see *Census of Great Britain, 1851*, pp. xxii, xxiii, xxvi.

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Period	England and Wales Estimated population (nearest 1,000)	Average annual rate of change, per cent
1700	5,307,000	
1750	5,895,000	+ 0·2
1801	9,337,000	+ 1·1
1851	17,719,000	+ 1·8

Although we have indicated that the marriage-rate was only stable during the eighteenth century, it is possible to check the earlier population estimates with estimates derived from independent source. Gregory King estimated the population of England and Wales to be 5·5 millions in 1695, an estimate which Professor Glass thinks may be slightly too high.[1] King's estimate was based on hearth-tax returns and local censuses conducted in connection with the tax on marriages etc.; it is similar to the one we have made for 1700 on the basis of the marriage returns.

The population increased relatively slowly up to 1750, after which it increased rapidly and steadily right through to the end of the nineteenth century. It is the causes of this rapid and consistent increase which is the subject of this paper.

The Age at Marriage of Spinsters, 1615–1841

Period	Region	Mean age at marriage	Number in sample
1615–21	Wilts., Berks., Hants and Dorset [2]	24·6	280
1662–1714	Yorkshire [3]	23·76	7,242
1701–36	Nottinghamshire [4]	24·5	865
1741–45	Surrey [5]	24·9	333
1749–70	Nottinghamshire [4]	23·9	700
1796–99	Sussex [6]	24·1	275
1839–41	England and Wales [7]	24·30	14,311

Ideally, we should want to analyse the aggregate birth- and death-rates, age-specific fertility and mortality-rates, etc. Unfortunately, the paucity of accurate information means that we can only collect data of a piecemeal kind, which at least points in the direction of certain conclusions. It has already been indicated that the aggregate marriage-rate changed but little during the eighteenth century. This conclusion is consistent with the fact that the age at marriage of spinsters was virtually constant during the same period. Our findings indicate that the population explosion in the eighteenth century was

[1] D. V. Glass, 'Gregory King's Estimate of the Population of England and Wales, 1695', *Population Studies*, III (1950), p. 358.

[2] Rev. E. Nevill (ed.), *Marriage Licences of Salisbury, 1615–1682*.

[3] M. Drake, 'An Elementary Exercise in Parish Register Demography', *Econ. Hist. Rev.* 2nd ser. XIV, (1962), p. 444.

[4] T. M. Blagg and F. A. Wadsworth (eds.), 'Nottinghamshire Marriage Licences', *The Index Library, British Record Society*.

[5] A. R. Bax (ed.), *Allegation for Marriage Licences Issued by the Commissary Court of Surrey, 1673–1770* (Norwich, 1907).

[6] D. Macleod (ed.), 'Sussex Marriage Licences, 1775–1800', *Sussex Record Society*, XXXV (1929).

[7] *Fourth Annual Report of the Registrar General* (1842), p. 10.

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not caused by a lowering of the age at marriage or by an increase in the marriage-rate due to any possible increase in the standard of living, or the level of employment associated with the Industrial and Agricultural Revolutions.

Another source of demographic information is to be found in life-expectancy tables. These were constructed for a group of Northamptonshire and Hertfordshire 'county families'.

Changes in the Average Age Lived (County Families) [1]

Cohort born	Expectation of life at birth (males)	Number of sample
1681–1730	37 years	138
1731–1780	48 years	130
1781–1830	50 years	162

The results of this study were compared with those published by Hollingsworth in his paper on the demographic history of ducal families,[2] as well as the results of his unpublished research into the whole of the aristocracy. All these studies point to the same conclusion: that expectation of life for cohorts born from *circa* 1740 onwards rose significantly, the saving of life occurring mainly amongst infants, children and young adults. A more detailed analysis of the 'county family' material illustrates the sharpness of this rise.[3]

Changes in the Average Age Lived (County Families)

Cohort born	Expectation of life at birth (males)	Number in sample
1680–99	36 years	92
1700–19	38 years	89
1720–39	35 years	86
1740–59	48 years	76

Unfortunately it is impossible to construct similar tables for the general population during the same period.[4] It is probable that there was a similar rise amongst the general population, for the mean expectation of life at birth derived from Gregory King's life-table for Lichfield in about 1695 was 32·0 years,[5] whereas according to the English life-table constructed by Farr in 1841 it was 41·2 years.[6] If these figures are representative, the aristocracy and gentry always had a higher life expectancy than the general population, but managed to increase their relative advantage slightly throughout the eighteenth and early nineteenth centuries.

[1] Samples were taken from the Northants and Herts. genealogical volumes of the *Victoria County History* series published in 1906 and 1907. Figures were computed to the nearest year.

[2] T. H. Hollingsworth, 'A Demographic Study of the British Ducal Families', *Population Studies*, XI (1957).

[3] Hollingsworth's figures for the whole aristocracy, which are based on much larger cohorts, indicate that the rise in life expectancy was somewhat more gradual than this.

[4] Although Finlainson analysed mortality-rates from annuities and tontines, his findings apply essentially to the aristocracy and gentry. His results confirm those of Hollingworth's two studies and my own, i.e. there was a very sharp rise in the expectation of life beginning sometime during the middle of the eighteenth century. See 'Report of John Finlainson on the evidence and elementary facts on which the Tables of Life Annuities are founded', Parl. Pap. 1829 (3).

[5] See Glass, *op. cit.* p. 368, for the reliability of this figure.

[6] *Fifth Annual Report of the Registrar General* (1843), p. 29.

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What are the possible causes of the increase in expectation of life throughout the eighteenth century? For obvious reasons, an explanation in terms of increased food supplies is inappropriate for social groups such as the gentry and aristocracy. The rise in expectation of life was too rapid amongst the 'county families' to be explained in terms of changes in environment. There is, however, one major plausible explanation: the introduction and use of *inoculation* against smallpox during the eighteenth century. Inoculation must formally be contrasted with the nineeteenth-century practice of vaccination. Inoculation is the injection of smallpox virus taken from the vesicle of a person suffering from smallpox, whereas vaccination is the injection of cowpox virus. The two injections are conventionally distinguished by the different symptoms they produce. Inoculation is thought of as giving rise to pustular eruptions in different parts of the body as well as at the site of injection and is viewed as a mild form of natural smallpox, inasmuch as it is believed to spread the natural disease from the inoculated person to other unprotected people. Vaccination only gives rise to a vesicle at the site of the injection and is not infectious to other unprotected people.

II

Inoculation was originally practised sporadically and on a very limited scale as a part of folk medicine, mainly in Oriental and African countries. It was introduced into England in 1721 when Lady Mary Wortley Montagu had her daughter inoculated in London, although it had been known by report for some years previously. It was only practised on a very limited scale during the 1720's and 1730's, owing mainly to the fact that the very severe technique of inoculation caused several deaths. Between 1721 and 1728 there were 897 people known to have been inoculated, 17 of whom were suspected to have died from inoculated smallpox. In the early 1740's the practice was revived again mainly as a result of the use of a safer technique involving milder injections of virus. However, because the medical profession had elaborated inoculation from its original simplicity into a very complex operation involving both a fortnight's preparation and convalescence, often in a special isolation hospital, the practice became very expensive and was consequently restricted to the rich. Although the London Smallpox Hospital was founded in 1746 to offer charitable inoculations to the poor, most of its clients in the early period tended to be servants of the subscribers to the foundation of the hospital.

During the 1750's the overseers of the poor began to pay the cost of inoculation for all the poor within their parish; this usually took place as a response to the threat of a smallpox epidemic which provoked mass inoculation amongst all members of the parish. In addition to these mass inoculations there were many individuals who were inoculated at their own expense. Thus Kirkpatrick wrote in 1754: 'But since we have certain accounts that the populace, who were at first strongly predisposed against this practice, and who so rarely stop at the Golden Mean, are rushing into the contrary extreme; and go promiscuously from different distances to little Market Towns, where without any medical

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advice, and very little consideration, they procure inoculation from some operator, too often as crude and thoughtless as themselves...' [1] This popularization of inoculation was made possible by its cheapness through the activities of local surgeons and apothecaries.[2]

However, inoculation did not become really widespread until after the 1760's for, according to one source, only 200,000 people had been inoculated in England by 1766.[3] The main reason why inoculation was not more widespread was the occasional mortality still associated with the operation. This situation was changed in the 1760's when the Sutton family began to inoculate by injecting the minimal amount of virus into the arm with the very lightest of scratches. The result was that 'if any patienth as twenty or thirty pustules he is said to have the smallpox very heavy',[4] thus ensuring a negligible risk of death. The Suttons claimed in 1768 'that about fifty-five thousand had been inoculated by them since the year 1760; of which number only six had died'.[5] The 'Suttonian Practice' consisted of Robert Sutton, an apothecary and surgeon at Framlington Earl, Norfolk, and several of his sons, as well as a very large number of non-family partners; the practice extended to most counties and several foreign countries.[6] The most famous son was Daniel Sutton who, because of his very spectacular feats of inoculation,[7] was chiefly responsible for popularizing the Suttonian method. By the end of 1776 they claimed to have inoculated 300,000 people,[8] a claim which is very plausible in the light of the very large number of partners they had. They offered to inoculate the rural poor *gratis* on the condition presumably that the rest of the parish were also inoculated by them; certainly the Suttons appear in the account books of innumerable overseers who paid them for mass inoculations in their parishes.

The Suttonian method was soon taken up by the rest of the medical profession, as well as by amateur inoculators who began to proliferate very rapidly. Thus Houlton wrote in 1768 'that in every county of England you meet advertisements of these pretenders and itinerants... Some of them as before observed, advertise that they inoculate according to the *Sutton method*; while others have the modesty to deck their imposition with the style of "*The Suttonian art improved*"...'[9] Some of these 'pretenders and itinerants' were undoubtedly professional surgeons and apothecaries such as Dimsdale who was converted to the Suttonian method by its superiority over the older technique;

[1] J. Kirkpatrick, *The Analysis of Inoculation* (1754), pp. 267, 268.

[2] This was achieved through the simplification of inoculation, culminating in the abandonment of preparation and convalescence by Lewis Paul Williams (a Leicestershire surgeon) in 1763. See *Northampton Mercury*, 15 Dec. 1768; *The British Medical Journal*, 11 (1910), pp. 633–34.

[3] See A. C. Klebs, 'The Historic Evolution of Variolation', *Bulletin of the Johns Hopkins Hospital*, XXIV (March 1913), 82. The basis of this estimate is unknown.

[4] Creighton, *op. cit.* p. 476.

[5] R. Houlton, *Indisputable Facts, Relative To The Suttonian Art Of Inoculation* (Dublin, 1768), p. 10. The negligible risk of death from inoculation after the 1760's is confirmed by a great deal of evidence.

[6] *Ibid.* pp. 21–23.

[7] During a mass inoculation at Maldon, Essex, he inoculated 487 people in one day, none of whom died.

[8] W. R. Clayton, 'Notes on the history, incidence and treatment of smallpox in Norfolk', *Norfolk Archaeological Society*, XXX 7.

[9] Houlton, *op. cit.* p. 24.

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another professional medical practitioner who later inoculated with the Suttonian method before discovering vaccination was Edward Jenner, who had been inoculated in the old method as a boy during the mass inoculation at Wootton-under-Edge in 1756. Others of the imitators of the Suttonian method were 'a certain tribe of empirics and other unexperienced Practitioners',[1] such as the livery servant who left his employment in about 1768 to become a full-time inoculator [2] and the farrier and blacksmith who inoculated 170 people in the neighbourhood of Norwich in 1769.[3] The occupations of the amateur inoculators ranged from farmer to customs-officer, and some set up schools in their own method of inoculation.

Inoculation was practised much more extensively and earlier in rural areas and small towns than in large towns and cities. Haygarth, writing in 1780, stated that 'whole villages in this neighbourhood (Chester) and many other parts of Britain, have been inoculated with one consent. And it cannot be supposed that the inhabitants of towns are more ignorant or more obstinate. There is not a reasonable doubt that our poor fellow citizens would eagerly and universally embrace a proposal to preserve their children from death and deformity, if the intelligent and the opulent would humanely exert their influence and assistance to carry it into execution'.[4] Although the relative lack of provision of charitable inoculation was one of the major reasons why it spread only slowly in the large towns, another reason was because of the differing structure of smallpox epidemics in town and countryside. In the large towns where the disease was endemic all smallpox deaths were of infants and young children; this tended to engender a fatalistic attitude about the inevitability of catching the disease. This was recognized by Haygarth when he wrote that 'the lower class of people (in Chester) have no fear of the casual (natural) smallpox. Many more examples occurred of their wishes and endeavour to catch the infection, than to avoid it. This... prejudice... probably prevails in other large towns, especially in those which are so large as perpetually to nourish the distemper, by so quick a succession of infants as constantly to supply fresh subjects for infection...'.[5] This he contrasted with 'small towns and villages, especially where placed in remote situations, the young generation grow up to have a consciousness of the danger before they are attacked by the dreadful disease'.[6] This consciousness was also based on the greater fatality of smallpox in isolated areas. One of its results was seen at Blandford, Dorset, in 1766 when a very malignant epidemic of smallpox broke out and 'a perfect rage for inoculation seized the town'.[7] In the small town or village it was possible for everybody to compare the spectacular differences in mortality of the inoculated and uninoculated during a smallpox epidemic, whereas in a large town it was very difficult to familiarize the poorer classes

[1] M. G. Hobson, *Otmoor and its Seven Towns* (Oxford, 1961), p. 20.
[2] W. Watson, *An Account ... of Inoculating the Small Pox* (Dublin, 1768), pp. 71, 72.
[3] *Gentleman's Magazine*, XXXIX (1769), p. 167.
[4] J. Haygarth, *An Enquiry How To Prevent The Smallpox* (Chester, 1785), p. 164.
[5] J. Haygarth, *A Sketch of A Plan to Exterminate the Casual Smallpox* (1793), p. 186.
[6] *Ibid.* p. 186.
[7] Creighton, *op. cit.* p. 513.

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with the benefits of inoculation owing to the dispersed and piecemeal nature of smallpox mortality.

The relatively slow spread of inoculation in the large towns must not be exaggerated in importance, for only a small minority of the total population lived in such areas. Also it appears that inoculation was making rapid headway in the large towns by the very end of the eighteenth century.[1] In the small towns and villages inoculation appears to have been universally practised well before the end of the century. There are innumerable references to mass inoculations in local histories and medical writings for every decade from about 1750 onwards.[2] One of the reasons why parish authorities were so willing to pay for inoculation of their poor was because of the great expenses involved in isolating and nursing the sick during an epidemic of the natural smallpox. The costs were sufficiently great to make many parishes *compel* everyone within their jurisdiction to be inoculated.[3]

One observer noted in 1771 'that inoculation, which was heretofore in a manner confined to people of superior ranks, is now practised even in the meanest cottages, and is almost universally received in every corner of this kingdom'.[4] According to Dimsdale, writing in 1776, 'in the county of Hertford, there have been two methods of public or general inoculation; one to inoculate, at a low price, as many of the inhabitants of any small town or village, as could be persuaded to submit to it, and at the same time were able to pay, refusing all those who had it not in their power to procure the money demanded. The other method has been, where the inhabitants of a town, or a district, of all denominations, have agreed to be inoculated at the same time, the parish officers or some neighbouring charitably disposed persons, having first promised to defray the expense, and provide subsistance for such of the poor, as unable to pay for themselves.'[5] To some extent the emergence of the amateur inoculators served the needs of the poor who were unable to afford the price of professional inoculation and whose parish was unwilling to pay for a mass inoculation. A supporter of inoculation summed up the extent of the practice by writing in 1805 that 'smallpox inoculation was a well-known, proved, and absolute prevention from receiving the *natural Smallpox* infection, as millions of people who living can testify'.[6] Inoculation did not disappear with the introduction of vaccination. On the contrary it remained very popular, especially with the poorer classes who were very prejudiced against vaccination. Ironically, inoculation and vaccination appeared to have supplemented one another in that virtually all of the population during the first half of the

[1] Many of these large towns founded dispensaries during the late eighteenth century which provided charitable inoculation. Although the London Smallpox Hospital only inoculated 36,378 people between 1746 and 1805, practitioners such as Daniel Sutton specialized in the inoculation of 'the families of artificers, handicraftsmen, servants, labourers, etc.' in the Metropolis.

[2] See the Appendix.

[3] See S. and B. Webb, *English Local Government – English Poor Law History*, I (1927), 306. M. F. Davies, *Life in an English Village* (1909), p. 74. E. G. Thomas, *The Parish Overseer in Essex, 1597–1834* (London M.A. Thesis, 1956), p. 394.

[4] *Medical Transactions*, II (1772), p. 279.

[5] T. Dimsdale, *Thoughts On General and Partial Inoculations* (1776), p. 29.

[6] W. Rowley, *Cowpox Inoculation no Security Against Smallpox* (1805), p. 4.

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nineteenth century were protected by one injection or the other, sometimes by both.[1] Inoculation was eventually banned by law in 1840 at the instigation of the supporters of vaccination who accused inoculation of spreading natural smallpox to the unprotected.

Inoculation was very extensively practised in other countries, several of which encouraged it by legal enactments during the latter half of the eighteenth century, e.g. Sweden, Russia and Austria. It appears to have been particularly popular in Ireland where itinerant tinker inoculators proceeded 'from village to village several times during the year for the purpose of *inoculating* the infantile population'.[2]

III

In order to determine the significance of inoculation it is necessary to discuss the history of smallpox mortality before its effective introduction. By smallpox mortality we mean the proportion of every 100 children born who died from the disease during their lives. There are two methods of estimating such smallpox mortality: (1) multiplying the extent of the disease by its case-fatality rate (allowing for children who would have died before they had a chance to catch the disease); (2) counting the number of smallpox deaths and expressing it as a proportion of the number of births, such information being occasionally found in parish registers – in a period of static population growth the proportion of smallpox deaths to all deaths will approximate the ratio of smallpox deaths per number of births. In order to estimate smallpox mortality we will use both methods outlined above. First, however, it is necessary to discuss the problem in interpreting smallpox statistics.

There are five major difficulties in using figures of smallpox mortality: (1) The existence of a type of smallpox, known as fulminating smallpox, which does not manifest the classical pock symptoms because of the rapidity with which it kills its victims. It has only been discovered relatively recently, for as a current medical authority on smallpox has observed, 'this is "sledge-hammer" smallpox, and the diagnosis both clinical and at autopsy is impossible unless smallpox is thought of and unless laboratory facilities are available and used to grow the virus'.[3] It is impossible to estimate what proportion of all smallpox deaths were of the fulminating kind; generally it would be highest in very isolated communities which lacked a pool of antibodies derived from frequent epidemics. (2) The variation in fatality of smallpox in different types of area. This was recognized by Lettsom when he wrote 'that in some countries, and even some counties of England, the infection does not appear for the space of some years; but when it does appear, it is more fatal; owing probably to this,

[1] See Dr J. Forbes, 'Some Account of the Small Pox lately Prevalent in Chichester and its Vicinity', *London Medical Repository* (September 1822), pp. 211–15, for an invaluable description of the history of inoculation and vaccination during the first two decades of the nineteenth century. Vaccination was not introduced into the area until 1812, although all the population appeared to have been protected by inoculation at least as early as the beginning of the nineteenth century.

[2] W. Wilde, 'Report on Tables of Deaths', *Population Census of Ireland 1851*, P.P., 1843 (24), p. xii.

[3] C. W. Dixon, *Smallpox* (1962), p. 9.

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that in great towns the infection being always prevalent, it is caught without the accumulated changes of air peculiarly favourable to epidemics; whereas, when it comes at stated periods, its malignity seems to be augmented by some unknownbut deleterious state of the atmosphere.[1] This, we now know, was due to the creation of a pool of antibodies in the large towns through constant recurrence of smallpox epidemics, which it has already been noticed occurred to a lesser extent in isolated areas. (3) A large number of smallpox deaths were unregistered for other reasons. Lettsom, who had a great deal of experience with the health of the poor in London, estimated that smallpox mortality was nearly twice that recorded in the Bills of Mortality, 'the genetic article "convulsions" having swallowed up, in his opinion, a large number of the smallpox deaths of infants'.[2] Very young infants are known to be vulnerable to fulminating smallpox[3] – and it appears that this could be partly the explanation of this mis-registration.[4] Lettsom also pointed out that from smallpox 'some have been deprived of sight; many have been afflicted with the evil and scrofulous complaints, to which they had previously been strangers; many have been disabled in their limbs ... at length, emaciated and debilitated, they have sunk under their miseries, and filled up the amazing list of consumptions; many of which originated from the violence of Natural Smallpox'.[5] Smallpox mortality was also much higher when the disease converged with epidemics of other diseases; some of the increased mortality would be ascribed to the other disease. (4) Pregnant women are particularly vulnerable when attacked by smallpox,[6] the great majority of their children dying because of such an attack. According to Dixon 'in forty-six cases where the infant's condition is recorded (when the mother has been attacked by smallpox), twenty-six were stillborn, and of the twenty born alive, eleven died later'.[7] Most of the stillborn children and many of those infants which died soon after birth were probably not recorded in the parish registers as they would not have been baptized; those deaths which were recorded were probably attributed to some causes other than smallpox, e.g. convulsions. Also according to a doctor of the Bristol Royal Infirmary during the middle of the eighteenth century, 'the female sex whose cases from about 12 years of age to 50 become more dangerous on account of their menstrual discharges, which sometimes coming on in the beginning or State of the Disease proves fatal'.[8] Thus the group of potential mothers was particularly vulnerable to death from smallpox, a fact that we shall discuss later in connexion with changes in the birth-rate. (5) Many people who died of smallpox appear to have been buried in non-consecrated

[1] T. J. Pettigrew, *Memoirs of the Life and Writings of the late John Cockley Lettson* (1817), II 121, 122.
[2] Creighton, *op. cit.* p. 534.
[3] Dixon, *op. cit.* p. 324.
[4] See J. Haygarth, *A Sketch of a Plan to Exterminate the Casual Small-Pox* (1793), p. 141: 'The disease most fatal is to infants in convulsions, arising from various causes; one of them is the small-pox. The two circumstances will explain the reason why, under one year old, the proportion of deaths by the smallpox is less than in subsequent periods....'
[5] Pettigrew, *op. cit.* I, 6.
[6] Dixon, *op. cit.* p. 326.
[7] Dixon, *op. cit.* p. 113.
[8] *Bristol Infirmary Biographical Memoirs*, I, 59.

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burial pits near the pest-houses or infirmaries used for isolating those sick of the disease. In the Maidstone parish register the incumbent summarized the burials for the year 1760 with the following entry: 'Total Burials – 223. Of the Small Pox from Dec. 13–59. besides. These carried out of Town 102.' It is quite clear from examining the average number of burials in Maidstone that these 102 smallpox victims were not a part of the total 223 burials, a conclusion confirmed by examining the ages of those buried in the churchyard. It is thought that they were buried out at the pest-house because it was quite common practice in the eighteenth century for hospitals to bury their own dead. Both the Northampton and London bills of mortality had yearly returns of the number of people buried in local infirmaries. People responsible for isolating and nursing smallpox victims were also considered responsible for burying them,[1] and this was because people were so terrified of smallpox that they feared the corpses themselves; there are references in the literature of incumbents refusing to perform the burial rites, and relatives refusing to attend funerals.[2] The existence of these non-consecrated burial grounds not only poses a problem for the construction of smallpox mortality statistics but also for those demographic studies which assume that burials entered in the parish register represent the total number of deaths.

We are now in a position to estimate total smallpox mortality. As earlier stated, there are two methods in arriving at such estimates, the first being to multiply the extent of smallpox by its case-fatality rate. As to the extent of the disease, most writers regarded it as a universal affliction to which all were subjected at some time or other, e.g. D'Escheray, in his writings on smallpox in England, observed in 1760 that 'this distemper spares neither Age nor Sex, Rich and Poor are equally exposed to its influence. What is the most unaccountable, and so wide from all other fevers, is, that the Difference of Constitution is no preservative against its Attack, insomuch, that very few escape it, at one time or other.'[3] This universality of smallpox is consistent with what we know about the nature of the disease; e.g. Dr J. F. D. Shrewsbury, the bacteriologist, has written that smallpox is 'the most highly infectious of the transmissible diseases of man'.[4] It appears from statistical evidence that smallpox was endemic in London as early as at least the sixteenth century; in fact the disease was so endemic as to be found regularly every week in the bills of mortality during the seventeenth and eighteenth centuries. Smallpox deaths occurred in other large towns during the eighteenth century at least every year. Thus London, and other large towns to a lesser extent, were smallpox reservoirs from which the disease was constantly exported to the countryside.

The case-fatality rate of smallpox may be estimated from a series of smallpox censuses conducted during the 1720's. The figures compiled were for the number of total cases of smallpox sickness with the resulting numbers of deaths

[1] See, for example, W. Le Hardy (ed.), *Calendar to The Herts Session Books, 1752–1799*, VIII (Hertford, 1935), 226.
[2] See, for example, Document I.C. 1185, 1679 in the Northampton Record Office.
[3] D. D'Escheray, *An Essay On The Smallpox* (1760), p. 2.
[4] Private communication, 1964.

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in thirty places. Of the 13,192 cases of people suffering from smallpox, 2,167 died i.e. an average case-fatality rate of 16·5 per cent.[1] This figure must be interpreted in the light of the difficulties in using smallpox statistics that we have already discussed. Three of the difficulties are relevant: (1) the figures would exclude cases of fulminating smallpox, the mortality from which is nearly 100 per cent; (2) large numbers of unregistered deaths would have been excluded, in the ways described by Lettsom; (3) variations in the fatality of smallpox varied from one type of area to another. With reference to the last difficulty, most of the censuses were conducted in market towns, many of them in Yorkshire and centres of industrial activity. These were towns of very frequently recurring epidemics, which consequently had a lower case-fatality rate than places like the isolated villages in Worcestershire studied by Eversley.[2] He has written that during the smallpox epidemic of 1725–30 in the area of Bromsgrove 'a conservative estimate of the net loss of population at Hanbury is 164 out of the 716 alive in 1715'.[3] This was similar to the epidemics in the Shetland Islands where 'formerly the smallpox occasioned the most dreadful ravages in these islands frequently carrying off a fifth part of the inhabitants', [4] 'in 1720, the disease was so fatal as to be distinguished by the name of the mortal pox. On this occasion tradition tells us, in the remote Island of Foula, probably inhabited by about two hundred people, it left only four to six to bury the dead'.[5] This type of spectacular smallpox mortality was to be found in other extremely isolated places where the population had no pool of antibodies to protect them.[6] It was noted by one contemporary medical observer 'that when the smallpox is epidemic, entire villages are depopulated, markets ruined, and the face of distress spread over the whole country'.[7] Certainly epidemics of the fatality of the one in Hanbury occurred quite often.[8] As about 23 per cent of the total population of Hanbury was wiped out, the case-fatality rate must have been considerably higher than this, for many of the older members of the village must have had smallpox when they were younger. Thus it appears that the case-fatality rate of 16½ per cent derived from the smallpox censuses in the market towns is much too low for the country as a whole. It is impossible to estimate total smallpox mortality for the whole countryside using the present method; suffice it to say that smallpox was a universal disease with a recorded case-fatality rate varying from 16½ to 97 per cent.

The other method of estimating smallpox mortality is to use the parish

[1] For details of the censuses see Creighton, *op. cit.* 518, 519.

[2] D. E. C. Eversley, 'A Survey of Population in an Area of Worcestershire', *Population Studies*, X (1956–57).

[3] *Ibid.* p. 267.

[4] J. Sinclair, *The Statistical Account Of Scotland*, II (1792), 569–70.

[5] Robert Cowie, *Shetland: Descriptive & Historical* (Aberdeen, 1871), pp. 73–75. See also Sinclair, *op. cit.*: XX (1798), 101, for another description of this epidemic.

[6] See E. W. and A. E. Stearn, *The Effect of Smallpox on the Destiny of the American Indian* (Boston, U.S.A., 1945); also *Royal Commission on Vaccination*, 1st Report (1889), pp. 109, 110.

[7] James McKenzie, *The History of Health* (1760).

[8] See the *Parish Register of Burford* in 1758; also *Gentleman's Magazine*, XLII (1772), 542. Many of the mass inoculations suggest that a very large proportion of village populations were vulnerable to smallpox, e.g. at Irthlingborough, Northants, 'upwards of Five Hundred People' were inoculated in 1778, whereas the total population was only 811 by 1801.

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registers and bills of mortality. Ideally, we would like to express the number of smallpox deaths as a proportion of the number of births. This is not always possible because of the lack of information about births, the deficiencies in registration, etc. When it is not possible, the proportion of smallpox deaths to all deaths will be used, as it will generally approximate the smallpox deaths/births ratio because of the relatively equal number of births and deaths during a period of static population. The smallpox mortality-rate in the eighteenth century varies from 11·6 smallpox deaths per 100 births in London during 1730–39,[1] 20 per 100 deaths in Dublin during the two approximate 30-year periods 1661–90 and 1715–46,[2] to an extreme proportion of 50 per 100 deaths in Great Chart, Kent, during 1688–1707.[3] The majority of records (mainly for towns) yield an average figure of about 15 per cent of all births and deaths due to smallpox during the first half of the eighteenth century. All of the difficulties outlined earlier in the paper apply to these statistics and all of them would tend to increase actual smallpox mortality over recorded mortality, e.g. Lettsom's estimate of the true smallpox mortality in London would raise the figure for 1730–39 from 11·6 smallpox deaths per 100 births to over 20 per 100, this being in an area where smallpox mortality was at its lowest due to the endemic nature of the disease. Once again it is impossible to estimate exactly the magnitude of smallpox mortality, but for the time being it will be sufficient to note that recorded smallpox deaths accounted for between 11·6 and 50 per cent of all those born and dying, and that actual smallpox mortality was possibly twice as large as that actually recorded.

IV

Why has the possibility of inoculation reducing smallpox mortality been rejected by previous historians? The two basic reasons for rejecting the effectiveness of inoculation have been: (1) the argument that inoculation spread natural smallpox to the unprotected; (2) the continuance of smallpox deaths in the bills of mortality of some of the large towns.

There are several reasons why the objection that inoculation spread natural smallpox is spurious: (*a*) smallpox was already a universal disease before the introduction of inoculation; (*b*) inoculation had become so widespread by the end of the eighteenth century that only a relatively small proportion of the population was left unprotected; (*c*) experimental and other evidence is available to show that inoculation did not spread natural smallpox to the unprotected. This conclusion is supported by the fact that *vaccination is in reality a more attenuated form of inoculation.*[4]

[1] J. Marshall, *Mortality of the Metropolis* (1832).

[2] J. Fleetwood, *History of Medicine in Ireland* (Dublin, 1951), p. 65; Dr J. Rutty, *A Chronological History of the Weather and Seasons, and of the Prevailing Diseases in Dublin* (Dublin, 1770).

[3] M. C. Buer, *Health, Wealth, and Population in the Early Days of the Industrial Revolution* (1926), p. 190.

[4] It is impossible in this paper to document this very controversial statement. The subject is of sufficient importance to warrant a separate paper. Suffice it to say that the inoculators were able to produce a single vesicle at the site of injection identical to that of vaccination, through a process of attenuation. Inoculation was superior to vaccination in that it conferred life-long immunity against further attacks of smallpox, owing to the larger amount of virus injected.

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Smallpox did continue to kill substantial numbers of children in some of the large towns during the late eighteenth century, but this has misled medical historians for two reasons: (*a*) the total population increased very rapidly in these places and if the number of smallpox deaths is expressed as a proportion of the number of children at risk, a marked reduction in smallpox mortality is seen to have taken place; (*b*) as we have already seen, these large towns were atypical in that inoculation spread much later in them than elsewhere. This was stated quite explicity by Howlett in 1781: 'It may be thought, at first sight, that the healthiness of London is more increased than that of country towns... But it must be remembered that the diminished mortality in the latter appears to be chiefly owing to the salutary practice of inoculation; whereas in the former, for want of universality, it has hitherto been of little advantage... In provincial towns and villages, as soon as this disorder makes its appearance, inoculation takes place amongst all ranks of people; the rich and poor, from either choice or necessity, almost instantly have recourse to it; and where two or three hundred used to be carried to their graves in the course of a few months, there are now perhaps not above 20 or 30.'[1]

Smallpox Mortality at Maidstone, 1754–1801 [2]

Period	Smallpox burials	All burials
1752–63	252	1,703
1762–71	76	1,426
1772–81	60	1,549
1782–91	91	1,676
1792–1801	2	2,068

An illustration of this reduction of smallpox mortality is to be found at Maidstone in Kent.

A mass inoculation was conducted by Daniel Sutton in 1776 and its effects were described by Howlett in a pamphlet by him in 1782. 'Upon casting an eye over the annual lists of burials, we see that, before the modern improved method of inoculation was introduced, every 5 or 6 years the average number was almost doubled; and it was found upon enquiry, that at such intervals nearly the smallpox used to repeat its periodical visits... in the short space of 30 years it deprived the town of between five and six hundred of its inhabitants; whereas in the 15 or 16 years that have elapsed since that general inoculation it has occasioned the deaths of only about 60. Ample and satisfactory evidence of the vast benefits the town has received from that salutary invention.'[3] There are many other statistical tables which can be produced to prove the effective-

[1] Rev. J. Howlett, *An Examination of Dr Price's Essay on The Population of England and Wales* (Maidstone, 1781), p. 94.

[2] Taken from the *Parish Register of Maidstone*, lodged in All Saints, Church, Maidstone. Smallpox deaths disappeared from the register after 1797. This gradual decline of smallpox cannot be attributed to a decrease in the virulence of the disease as all the evidence points to the opposite conclusion, i.e. an increase in its virulance, e.g. the case-fatality rates at the London Smallpox Hospital were as follows in 1746–63, 25%; 1775–99, 32%; 1836–1856, 35%. See the *Royal Commission on Vaccination*, 1st Report (1889), p. 74 and the *Royal Commission on Vaccination*, 3rd Report (1890), p. 100.

[3] J. Howlett, *Observations On The Increased Population... Of Maidstone* (Maidstone, 1782), p. 8.

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ness of inoculation,[1] the most detailed being for Boston, U.S.A. during the eighteenth century, from which it is possible to attribute the reduced mortality directly to inoculation.[2]

The effects of inoculation were described in contemporary literature; e.g. in *She Stoops To Conquer* written in 1773, Mrs Hardcastle says to Hastings: 'I vow since Inoculation began, there is no such thing to be seen as a plain woman. So one must dress a little particular; or one may escape in the crowd.' Arthur Young, writing an essay on population in 1781, wrote: 'In several of these parishes where population had for some periods been rather on the decrease, a great change has taken place lately, and the last ten years are found to be in a rapid state of progression; as considerable drains of men have been made from almost every parish in the kingdom for the public service in that period, I should not have expected this result, and know nothing to which it can be owing, unless the prevalence of inoculation, which certainly has been attended with a very great effect.'[3] There are also references to the effects of inoculation on mortality in the reports on agriculture made by local observers to the Board of Agriculture at the end of the eighteenth century, e.g. 'I may further add, that since the year 1782, when these observations were made, the population of this parish has been increasing: most certainly inoculation for the Smallpox... has been most essential to population throughout this kingdom'.[4] Similarly John Holt of Lancashire wrote in 1795: 'One reason, why persons in large manufacturies in Lancashire, do not frequently die in great numbers... is that they have (in general) been inoculated in their infancy. Inoculation is the most effectual of all expedients for preserving the short lived race of men – many gentlemen pay for inoculation of the children of the poor in their own neighbourhood.'[5]

In 1796 it was observed that 'the increase of people within the last 25 years is visible to every observer. Inoculation is the mystic spell which has produced this wonder... before that time it may be safely asserted, that the malady, added to the general laws of nature, did at least equipoise population. It is now 30 years since the Suttons and others under their instructions, had practised the art of inoculation upon half the kindgom and had reduced the chance of death to 1 in 2,000.'[6] Similarly another gentleman observed later in 1803 that 'one very great cause of increasing population may be ascribed to the

[1] For the sources of these statistics see: the parish registers of Basingstoke (Hants), Calne (Wilts), Milton Ernest (Beds), Whittington (Salop), Selattyn (Salop), Boston (Lincs). For other statistics see 'An Abridgement of the Observations on the Bills of Mortality in Carlisle, 1779–1787' by Dr Heysham in W. Hutchinson, *The History of Cumberland* (Carlisle, 1794), pp. 668–75.

[2] The number of inoculations in this town increased from 287 in 1721 to 9,152 in 1792, which was the vast majority who had not had smallpox before. Smallpox mortality fell from 175 smallpox deaths per 1,000 living population in 1677–78 to 10 per 1,000 in 1792, and this was in spite of the fact that the virulence of the disease generally increased throughout the period. See J. Blake, *Public Health in the Town of Boston (Mass.), 1630–1822* (Cambridge, U.S.A. 1959), p. 244. H. R. Viets (Ed.), *A Brief Rule to Guide the Common People of New England* (1937) p. xxxv. *Royal Commission on Vaccination*, 6th Report', Parl. Pap. 1896 (47), p. 762.

[3] A. Young, *Annals of Agriculture*, VII (1786), 455.

[4] J. Plymley, *General View of the Agriculture of Shropshire* (1803), pp. 343, 344.

[5] J. Holt, *General View of the Agriculture of Lancaster* (1795), p. 208, n.2.

[6] *Gentleman's Magazine*, LXVI, 1 (1796), n. 112.

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success of inoculation for the Smallpox. One in four or five, or about 200 to 250 in a thousand, usually died of this loathsome disorder in the natural way of infection... so that this saving of lives alone would account for our increasing number, without perplexing ourselves for any other cause.'[1]

It is necessary to attempt to evaluate the claims that some contemporaries made of the effect of inoculation on population growth. Unfortunately there is virtually no reliable demographic data available with which we can do this. An analysis of the 'county family' life tables suggests that a reduction of about 25 per cent in mortality amongst the younger age-groups could account for the whole increase in expectation of life between 1681–1730 and 1781–1830. The same conclusion probably applies to both the ducal familes and the whole of the aristocracy. For the population as a whole there is no data sufficiently reliable to test the hypothesis directly. However, it is possible to construct a simple hypothetical model whose limits are defined by the small amount of reliable information that we do possess. In 1697 Gregory King constructed a 'life table' for Lichfield; Professor Glass has written that 'it would appear that by taking Lichfield as a basis, King began with a collection of statistics which were probably not markedly untypical, and then adjusted more acceptably as an indication of national structure'.[2] It is possible by using King's 'life table' to construct a hypothetical population reproduction model for our period.

Female Population Reproduction, 1750–1855

		Numbers surviving in the following years							
		1750	1765	1780	1795	1810	1825	1840	1855
Numbers surviving to the following ages (years)	0	1000	1071	1237	1468	1762	2116	2538	3045
	15	620	680	793	952	1138	1366	1640	1967
	30	450	480	559	659	798	956	1146	1376
	45	315	325	357	422	498	603	722	866
	60	190	190	196	215	255	300	364	435
	75	50	50	50	52	57	67	79	85
	90	0	0	0	0	0	0	0	0
Population Index [3]		2125	2260	2573	3034	3627	4350	5220	6251

The above model was constructed on the following assumptions: (1) increase in the female population was proportionate to the increase in total population; this ignores the effects of the relationship between the number of males and females, e.g. the proportion of married women who were widowed; (2) of 1,000 female children born before 1750, the numbers surviving to various ages were the same as in King's 'life table'; (3) the population was static before 1750, based on an age-specific birth-rate of 1 female child born for every 13·7625 women living between 15 and 45; (4) the age-specific birth-rate remained constant throughout the whole period; (5) of every 1,000 born, lives were saved in the following manner:

[1] *Gentleman's Magazine*, LXXIII, 1 (1803), 213.

[2] D. V. Glass, 'Gregory King's Estimate of the Population of England and Wales, 1695', *Population Studies*, III (1949–50), 368.

[3] This population index is the sum of the average number of people living in each age-period, i.e. I have not bothered to multiply by 15 throughout.

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		Period			
		1750–65	1765–80	1780–95	1795–1810
Ages (years)	Under 15	60	60	20	15
	15–30	30	30	5	5
	30–45	10	10	5	0

In all, it is assumed that 250 lives were saved out of 1,000 born. According to our earlier estimates of population growth, it almost exactly trebled between 1750 and 1851. In our model it does not quite do this, but we assumed that population was static before 1750, whereas according to the earlier estimates it was increasing about 0·2 per cent per annum between 1700 and 1750. If an allowance is made for this pre-1750 growth, population in our model increases by 3·2 times between 1750 and 1851; the greater the allowance made for pre-1750 growth the more the model population increase will exceed that as estimated. The point of the model is not to describe exact changes in the population structure, but rather to estimate the magnitude of lives required to be saved in order to generate the rate of increase in estimated population. The assumptions are thought to be realistic because: (1) the crude birth-rate appears to have been very similar between the 1690's and the 1840's; [1] (2) the saving of life (250 out of 1,000 born) assumed is very similar to that which took place amongst the gentry and aristocracy.

In order for inoculation against smallpox to account for the whole of the population increase, smallpox mortality before inoculation must have been about 310 deaths per 1,000 born, for of the 250 lives saved of every 1,000 born in our model, about 45 would have died of other diseases during the same age-period, while smallpox accounted for about $1\frac{1}{2}$ per cent of deaths of all born during 1838–4,[2] when civil registration was first introduced. It is impossible to state definitely that smallpox mortality before inoculation was as high as 310 deaths per 1,000 born, but we may conclude from our earlier discussion that this is certainly a plausible figure. It must be remembered that much of this saving of life would have been indirect, insomuch as the elimination of smallpox attacks probably increased the expectation of life of those who did not die of the disease. Also the vulnerability of mothers and other young adult females to smallpox could have meant that the elimination of the disease led to an increase in the birth-rate; e.g. at Basingstoke (Hants) the average number of baptisms in the ten years before the smallpox epidemic in 1741 was 69·6, whereas in the following ten years it fell to 45·5 (a much greater fall than the average number of deaths and therefore presumably the population), which

[1] The birth-rate was estimated as 34·5 births per 1,000 living during the 1690's by Gregory King and 35·2 per 1,000 during 1841–45 by Professor Glass from civil registration returns. See G. King 'Natural And Political Observation 1696' in George Chalmers, *An Estimate of the Comparative Strength of Great Britain* (1804), p. 44; and D. V. Glass, 'A Note on the Under-Registration of Births in Britain in the Nineteenth Century', *Population Studies*, V (1951), 85. Professor Glass has written about the basis of King's estimate: 'the statistics collected were more comprehensive than any provided previously and, indeed, than any subsequent statistics prior to the establishment of the full mechanism of censuses and civil registration in the nineteenth century'. See D. V. Glass, 'Gregory King and the Population of England and Wales', *Eugenics Review*, 37 XXXVII (1946), 175.

[2] See Creighton, *op. cit.* This figure includes chickenpox deaths, which is assumed to approximate omissions due to fulminating smallpox, etc.

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was possibly due to the fact that one half of the smallpox deaths occurred amongst adults.[1] A rise in the age-specific birth-rate was not allowed for in the population reproduction model for two reasons: (1) simplicity and economy; (2) the very long term stability of the estimated crude birth-rate. Thus any increase in the birth-rate has been absorbed for analytical reasons into a fall in the death-rate.

Although it is not possible to analyse in any detail the history of other diseases, it is possible to draw some conclusions from bills of mortality. For example, in Northampton there was no major epidemic of any disease, other than smallpox, during the hundred-year period after 1736 when records were kept.[2] Smallpox epidemics occurred every seven years on average in Northampton before the introduction of inoculation; the listing of diseases and epidemics was very similar in a place like Maidstone; i.e. recurrent severe smallpox epidemics were the only causes of sharp rises in mortality-rates. This would indicate that the sharp peaks in mortality found in many local studies were due to smallpox and that they only disappeared with the introduction of inoculation.

Ideally one would like to trace the history of all diseases in order to evaluate their importance in contributing to total mortality, but unlike smallpox, most other diseases prevalent in the eighteenth century are not sufficiently distinctive to be analysed statistically. Many incumbents in their returns to Sir John Sinclair for the *Statistical Account of Scotland* discussed the history of diseases in their parish. No disease, other than smallpox (due to inoculation), was described as having declined or disappeared, except ague (malaria) which is very frequently mentioned as having disappeared during the latter half of the eighteenth century. Recently, one medical authority has questioned whether malaria was ever endemic in Britain.[3]

However, the incumbents so consistently mention that the disappearance of ague was linked with the draining of marshes, the reclamation of swamp-land etc., that one is led to suspect that the disease they described was malaria; this is confirmed by any descriptions of the disease that they give. Buer, in her discussion of malaria, maintained that although 'its direct effect on the death-rate was small, its indirect effect must have been great'.[4] Certainly it rarely appeared in the bills of mortality and parish registers as a cause of death even during the early eighteenth century. Malaria in England is a subject which warrants further investigation.

Although this paper has laid great stress on the importance of inoculation against smallpox as a cause of the population explosion during the eighteenth century, this does not rule out the role of other explanations.[5] However, while there is no convincing evidence for any of these other explanations we must provisionally reject them, and such a rejection can only be nullified by detailed

[1] See the *Basingstoke Parish Register*.
[2] See the *Northampton Bills of Mortality* in the British Museum Library.
[3] McKeown and Brown, *op. cit.* p. 124, n. 4.
[4] M. C. Buer, *op. cit.* p. 212.
[5] For example, the effects of the changing distribution of population between rural and urban areas has not been discussed in this paper.

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and plausible evidence to the contrary. Inoculation against smallpox could theoretically explain the whole of the increase in population, and until other explanations are convincingly documented, it is an explanation which must stand as the best one available.[1] Although the Industrial and Agricultural Revolutions did not cause the population explosion, they at least enabled population to grow unchecked. In Ireland where such Revolutions did not take place, the Malthusian check of mass starvation was the result of a rapidly increasing population without concomitant changes in the structure of the economy. The main achievement of the Industrial and Agricultural Revolutions in their earlier phases was the *maintenance* of the standard of living in a period when population was growing for reasons unconnected with the Revolutions themselves.

APPENDIX

In order to indicate the extent of mass inoculation, a sample was taken of those described in local histories, medical commentaries, accounts of the Overseers of the Poor, local newspapers, etc. The following list is in no sense comprehensive or representative, but merely a series of isolated examples culled from the literature, mainly from the South of England. The name of the town is given first, followed by the date of the mass inoculation:

Guildford, Surrey, 1740's. Salisbury, Wilts., 1751–52. Bradford-on-Avon, Wilts., 1752–53. Blandford, Dorset, 1753, 1766. Wootton-Under-Edge, Gloucs., 1756. First Regiment of Foot Guards, 1756. Beaminster, Dorset, 1758, 1780, 1791. Maldon, Essex, 1764. Maidstone, Kent, 1766. Marnham, Notts., 1767. Rye, Sussex, 1767. Neighbourhood of Norwich, 1769. Burton, Lincs., 1770. Berkhamstead and surrounding villages in Herts., 1770. Corsley, Wilts., 1773; Meopham, Kent, 1776. Bedford, Beds., 1777. Ware, Herts., 1777. Great Clivall, Essex, 1778. Irthlingborough Northants, 1778. Villages in the neighbourhood of Carlisle, Cumberland, 1779, 1781. Cricklade, Wilts., 1783. Painswick, Gloucs., 1786. Knowle, Kent, 1787. Weston(?), 1788. Northwold, Norfolk, 1788. Cowden, Kent, 1788. Luton, Beds., 1788. Bozeat, Northants., 1789. Chislehurst, Kent, 1790, 1799. Toddington, Beds., 1790, 1801, 1824. Weston, Norfolk, 1791. Eaton Socon, Beds., 1793, 1800, 1808. Hevingham, Norfolk, 1794. Berkeley, Gloucs., 1795. Hastings, Sussex, 1796–97. Dursley, Gloucs., 1797. Three villages near Gillingham, 1797. Tenterden, Kent, 1798. Rayne, Essex, 1806. Chichester, Sussex, 1806, 1812, 1821.

Under Dimsdale's influence, mass inoculations increasingly became 'general' rather than 'partial'.[2] General inoculations usually involved a degree of compulsion as was described by Cowper, the poet, in 1788: 'the smallpox has done, I believe, all that it has to do at Weston. Old folks, and even women with child, have been inoculated ... No circumstances whatsoever were permitted to exempt the inhabitants of Weston. The old, as well as the young, and the pregnant, as well as they who had only themselves within them, have been inoculated....'[3] An example of the

[1] This is particularly true with respect to the increase in expectation of life of the aristocracy and gentry.

[2] See, for example, T. Dimsdale, *Remarks on 'A Letter to Sir R. Barner...'* (1779), p. 13; and Walker, *op. cit.* p. 467, n.

[3] S. and B. Webb, *op. cit.* p. 306, n. 2.

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effects of general inoculation is to be found at Calne, Wilts. A local surgeon, Mr Wayte, described in 1795 a general inoculation as follows: 'in September, 1793, when the poor of the parish were inoculated... We inoculated six hundred and upwards... Besides the poor, I inoculated about two hundred (private) patients... Now in inoculating a whole parish, we have no choice of patients, all ages, and the sickly as well as others, were inoculated; but these were mostly children, as I assisted in inoculating the whole parish, about twelve or thirteen years ago.'[1] According to the Calne parish register the number of smallpox deaths declined as follows:– 1723–42 – 205; 1743–62 – 122; 1763–82 – 54; 1783–1802 – 8. The last mention of smallpox deaths is in 1793 when there were 6; previous to this there had been a very minor epidemic in 1782 involving 10 deaths (this was the epidemic which provoked the earlier general inoculation mentioned by Wayte). These late eighteenth-century epidemics should be compared with the major ones in the early eighteenth century, e.g. in 1732 there were 173 people registered as dying from smallpox.

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[1] Thomas Beddoes, 'Queries Respecting A Safer Method of Performing Inoculation' in Don A. De Gimbernat (Beddoes translated), *A New Method of Operating for the General Hernia* (London, 1795), pp. 56–59.

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P. E. Razzell

10 Population Growth and Economic Change in Eighteenth- and Early Nineteenth-Century England and Ireland

In recent papers on this important and controversial subject Professor Chambers has eloquently argued that although population growth and economic change were linked in eighteenth-century England the increase in population cannot be explained directly in economic terms.[1] This is a view no longer fashionable. As is well known, the traditional 'medical' explanations of a fall in the death rate have been discredited by medical historians, a conclusion that has led them to an assumption that economic growth must have preceded and 'caused' population expansion. In this essay I try to deal with some of the important problems raised by Professor Chambers, and attempt to demonstrate that the large increase in population during the eighteenth and early nineteenth centuries was in no way due to economic factors, but on the contrary was a major cause of economic change, which in England culminated in those changes known as the Industrial Revolution.

[1] See particularly J. D. Chambers, 'The Vale of Trent 1670–1800', *Econ. Hist. Rev.* Supp. 3. He concluded from this study that population 'was vulnerable to disease, but not as a result of famine. Epidemics could do their own work without its aid, nor it would seem, did they require the assistance of gin. . . For reasons which are far from clear, its [disease's] severity was mitigated from the middle of the [eighteenth] century in this region, especially in regard to the lower age groups. . . .'

I

The first point to be considered is whether the increase in population was due to a fall in the death rate or a rise in the birth rate. One of the most popular interpretations of the growth of population is the neo-Malthusian view that there was an increase in the birth rate due to expanding employment opportunities and a rise in the general standard of living, associated with the economic advances, encouraging earlier marriage and a higher marriage rate. However, there is evidence to suggest that both the age at marriage and the marriage rate were roughly constant throughout the eighteenth century.[1] Professor Chambers himself has published statistics for agricultural villages which suggest that both the birth and marriage rates may have declined between 1743 and 1801 in the Vale of Trent region.[2] In 1751 Thomas Short published statistics of population, baptisms, marriages, and burials during 1724–36 for seven market towns and fifty-four rural parishes.[3] According to his figures, the baptism rate was 33.8 per 1,000 and the burial rate 29.4 per 1,000; undoubtedly some births and deaths were not registered owing to the presence of Dissenters, particularly in the market towns. This, of course, would raise both the 'true' birth and death rates. If we compare these rates with those computed from civil registration returns in the 1840s, it is quite clear that the long-term birth rate was more or less constant, while there was a sharp fall in the death rate. The latter is also confirmed by the figures for agricultural villages published by Chambers.[4] One of the weakest points in the neo-Malthusian argument is that the fairly reliable figures of the 1840s indicate no particular association between the distribution of industry and high fertility rates. The counties with the highest age-specific birth and marriage rates and the lowest age at marriage during the early 1840s were Cambridge, Bedford, Huntingdon, and Northamptonshire, all largely agricultural counties; although Lancashire had a high crude

[1] The figures for the age at marriage are derived from marriage licences which are not entirely satisfactory. However, figures from parish registers suggest a similar conclusion. See C. C. Morell, 'Tudor Marriages and Infantile Mortality', *Journal of State Medicine*, XLIII (1935), p. 179.

[2] Chambers, *op. cit.*, p. 55. We do not have to take these figures too literally to conclude that the birth- and marriage-rates did not rise.

[3] T. Short, *New Observations on Bills of Mortality* (1751), p. 133.

[4] Chambers, *op. cit.*, p. 55. The reverse of these trends applied, however, in the town of Nottingham.

birth rate, its age specific birth rate and age at marriage appear to have been about average.[1] Furthermore, the age at marriage of spinsters appears to have varied little between different social strata during the eighteenth century, suggesting that economic considerations were not paramount in determining the age at marriage for women at least.[2]

It is difficult to draw any reliable conclusions from the statistics derived from the Anglican parish registers. The figures for burials are much more reliable than those for baptisms; this is because so few Nonconformists were buried outside the Anglican Church,[3] and the main reason for the under-registration of deaths was the existence of private burial grounds in the large cities.[4] If we exclude urban industrial counties from the analysis, it is clear that there was a substantial fall in the death rate during the eighteenth century,[5] not unlike that estimated by Talbot Griffiths.

In addition to this evidence, several recent studies of the aristocracy and gentry indicate that there was a sharp drop in mortality during the middle of the eighteenth century.[6] Hollingsworth's study of the

[1] The age at marriage in Lancashire was about the same as for the country as a whole. The ranking of age-specific birth-rates varies considerably according to which age group of women is considered; if the age group 20–30 is taken the age-specific birth-rate is below average, for the age group 15–45 it is above average. See *4th R. G. Report 1842* p. 9; *8th R. G. Report 1845* pp. 5, 37, 187, 191.

[2] The mean ages at marriage of spinsters calculated from the Nottinghamshire marriage bonds and allegations for the period 1701–70 were as follows (number in sample is given in brackets): Farmers and yeomen: 24 (285); Husbandmen: 24½ (235); Labourers and servants: 25 (390); Artisans and tradesmen: 23½ (290); 'Gentlemen': 24 (210).

[3] There were four baptism birth registers to one burial register kept by religious nonconformists before 1810. Few Methodists buried outside of the Anglican Church before 1810. See 'Report on Non-Parochial Registers', *Parl. Pap. 1837–38/28.*

[4] This was reflected in the death/burial ratios for different counties, e.g. the 1839–40 ratio for Lancashire was 1.61, as against the national average of 1.18. P. Deane and W. Cole, *British Economic Growth 1688–1959* (Cambridge, 1962), pp. 108, 109.

[5] According to the Deane and Cole figures, the death-rate in eighteen southern counties fell from 30.6/1,000 in 1701–50 to 20.6/1,000 in 1801–30. *Ibid.*, p. 127. Although these figures must not be taken too literally, the long-run trend is probably fairly accurately described by them.

[6] See my 'Population Change in Eighteenth-Century England. A Reinterpretion', *Econ. Hist. Rev.*, 2nd ser., XVIII (1965); T. H. Hollingsworth, 'A Demographic Study of the British Ducal Families', *Population Studies*, XI (1957); T. H. Hollingsworth 'The Demography Of The British Peerage', Supp., *Population Studies*, XVIII, No. 2 (1964).

aristocracy yielded the following increase of expectation of life at birth for females during the eighteenth century:

Table 1. Expectation of life at birth for aristocratic women.

1700–24	1723–49	1750–74	1775–99	1800–24
36.3	36.7	45.7	49.0	51.7[1]

Most of the increase in life-expectancy was due to the saving of life amongst younger age groups. These statistics are derived from sources sufficiently reliable for us to be sure that they describe a genuine sharp decline in mortality. Although it is not justifiable to generalize about the total population from such a finding, we must attempt to explain it in terms which might be relevant for the whole population. Obviously an explanation in terms of the quantity of food supply is irrelevant to groups such as the gentry and aristocracy. Mortality diminished so rapidly during 1750–74 that one must seek an explanation more radical than those usually given. It is my view that such an explanation is the effective introduction of inoculation against smallpox from about 1740 onwards.

The elimination of smallpox amongst the aristocracy could explain the whole of the rise in the expectation of life for that group,[2] and indeed for the whole of the increase in population during the late eighteenth and early nineteenth centuries. For the population as a whole inoculation only became popular after about 1765, when the Suttons perfected their much safer technique. Jenner himself recognized this, for he wrote 'that the common people were rarely inoculated for the smallpox, till that practice was rendered general by the improved method of the Suttons. . . .'[3] Howlett in 1782 collected statistics from 225 parishes for the two approximate periods 1734–53 and 1754–73; the balance of baptisms over burials in the first period was negligible,

[1] Hollingsworth, *op. cit.* (1964), p. 57.

[2] That inoculation was responsible for the elimination of smallpox, rather than vaccination, is supported by the negligible rise in life-expectancy for the aristocracy between 1800 and 1824.

[3] *The Medical Repository* (New York, 1803), V, 239. Chambers draws attention to payment by a Nottinghamshire parish to one of the Suttons for inoculating some poor children in 1767, *op. cit.*, p. 32 n. 4. He also notes a relatively slight smallpox epidemic occurring in Nottingham in 1801, which is not incompatible with the slow spread of inoculation in towns outlined in earlier papers. The same is to some extent true of Boston, Lincs. (mentioned by Chambers), where the decline of registered smallpox deaths was from 14.1 smallpox burials per 100 baptisms during 1749–75 to 5.25 per 100 during 1776–1802.

and was only slightly greater in the second, suggesting that the great increase in population occurred after 1770,[1] which fits in very well with the chronology of the spread of inoculation. Other medical and environmental 'improvements' were associated with the large towns, yet in 1801 only about a fifth of the total population lived in towns with a population greater than 10,000.[2] Even as late as the 1840s mortality in the large towns was very high: for example, about 48½ per cent of all males born in the Liverpool district died before the age of 5 during 1838–44[3] Any improvements in the large towns would have been more than outweighted by the consequence of a smaller proportion of the total population now living in the relatively healthy rural areas. Furthermore, the medical historians T. McKeown and R. G. Brown have pointed out that most of the medical 'improvements' during the eighteenth and nineteenth centuries, e.g. fever hospitals and midwifery services, were probably ineffective.[4] Even if they were effective it is doubtful whether they affected more than a very small minority of the total population.[5]

In the country as a whole smallpox was the only significant epidemic disease so far as mortality was concerned. For example, Charles Deering, the historian of Nottingham, wrote in 1751 that 'there mostly happens once in five Years some Distemperature in the Air, which either brings along with it some Epidemical Fever, (tho' seldom very Mortal) or renders the Small-Pox more dangerous than at other Times; of this last, the Year 1736, was a fatal Instance . . . the Burials exceeded that Year the Births by above 380. . . .'[6] Deering implied that smallpox occurred

[1] The exact figures are:

	Baptisms	Burials	Marriages
1734–53	109,478	104,750	34,110
1754–73	123,715	109,758	40,285

See J. Howlett, *Observations on The Increased Population, Healthiness . . . of Maidstone* (Maidstone, 1782), p. 14. This pamphlet was published anonymously and a copy of it is to be found in Maidstone Museum.

[2] See B. R. Mitchell and P. Deane, *Abstract of British Historical Statistics* (Cambridge, 1962), pp. 8, 24–27.

[3] *8th R.G. Report, 1846*, p. 206.

[4] T. McKeown and R. G. Brown, 'Medical Evidence Related to English Population Changes in the 18th Century', *Population Studies*, IX (1955–6).

[5] With reference to improvements in midwifery, the figures produced by Dr. Eversley for the Worcestershire area do not suggest any significant fall in infant mortality during the eighteenth and early nineteenth centuries; this finding is compatible with the high infant mortality rate (about 15 per cent) for England and Wales at the beginning of civil registration. See D. E. C. Eversley, 'A Survey of Population in an Area of Worcestershire from 1660–1850 on the Basis of Parish Records', *Population Studies*, X (1956–7), pp. 269–71.

[6] C. Deering, *Nottinghamshire Vetus Et Nova* (Nottingham, 1751), p. 82.

in Nottingham every five years or so, a cycle of epidemics that we know from bills of mortality and parish registers to be very similar to those in other towns like Northampton and Maidstone. He also pointed out that the 1736 epidemic was the most severe since the Plague. Smallpox was increasing in virulence throughout the seventeenth and eighteenth centuries, an increase which was particularly marked during the 1720s and afterwards. For example, the total number of smallpox deaths in Godalming, Surrey, was as follows: 1686, 50; 1701, 24; 1710–11, 39; 1722–3, 94.[1] This is the probable explanation for the check to population increase which occurred in the 1720s; although Creighton, the medical historian, mentions influenza as an important disease during this period, it never appears in bills of mortality and parish registers (under the label of fever) as accounting for large numbers of deaths during an epidemic, as does smallpox. An example of how misleading Creighton was on this question is to be found at Exeter in 1729. Creighton reported a rumour that the high mortality during that year was due to influenza, yet a local diarist did not mention the disease, but noted that 'The Small Pox was very fatall to some. Mr. Vivian lost all his children, being four sons.'[2]

It is possible, of course, that an improved standard of life diminished mortality amongst the general population, but such an explanation does not fit in with the chronology of population growth and *per capita* incomes. It is probable that it was during the first half of the eighteenth century, rather than the second, that any rise in real incomes of the labouring classes took place,[3] yet population increased much more rapidly at the end of the century. I have already pointed out that growing real incomes could hardly explain the sharp fall in mortality amongst the gentry and aristocracy; and further, there was surprisingly little variation in adult male mortality between different occupational groups, due to income differentials, during the middle of the nineteenth century,[4] suggesting that income factors were not important in determining rates of mortality.

2

The most recent comprehensive work on the history of Irish population during the eighteenth and early nineteenth centuries is that by Professor

[1] *Surrey Archeological Collections*, XXVII, pp. 16–20.
[2] See R. Pickard, *Population and Epidemics of Exeter* (1947), pp. 65, 66.
[3] For example, see Deane and Cole, *op. cit.*, pp. 19, 91.
[4] See the *14th R.G. Report 1851*, pp. XVIII, XXII.

Connell. He concluded that the great acceleration in population growth at the end of the eighteenth century was due 'very likely to the increase of fertility that followed earlier marriage'.[1] Dr. M. Drake, however, has recently criticized this interpretation on the ground that the statistics of the 1830s do not, in fact, indicate a low age at marriage.[2] The following statistics appear to support this criticism:

Table 2. Proportion Unmarried of 100 of the Population of the Respective Ages (Ireland, 1841)[3]

		Under 17	17–25	26–35	36–45	46–55	53 +
Males	Rural	100	93	44	16	16	8
	Civic	100	87	36	17	12	10
Females	Rural	100	81	28	15	12	12
	Civic	100	79	33	20	15	15

The distribution of the unmarried amongst various age groups was very similar to that in England at about the same time;[4] if one allows for the overstatement of early marriages in the statistics for the 1830s (as outlined by Drake), it would appear that the mean age of marriage of spinsters and bachelors was nearly the same for both Ireland and England, i.e. about 24½ for spinsters and 25½ for bachelors.[5] This finding agrees with the fact that both the crude birth rate and age-specific birth rate were similar for the two countries for the period around 1840.[6] It might be argued, of course, that the relatively late age

[1] K. H. Connell, *The Population of Ireland, 1750–1845* (Oxford, 1950), p. 248.

[2] M. Drake, 'Marriage and Population Growth in Ireland, 1750–1845', *Econ. Hist. Rev.*, 2nd ser., XVI (1963–4).

[3] Population Census Ireland 1841, *Parl. Pap. 1843/24*, pp. 41, 42. Indeed, Ireland appears to have had one of the highest mean ages at marriage and lowest marriage rates in Europe. The contradiction between the literary and statistical evidence was pointed out in *6th R.G. Report 1844*, pp. XXXIII, XXXIV.

[4] See Mitchell and Deane, *op. cit.*, pp. 15, 16.

[5] For English ages at marriage during 1839–41 see the *Fourth Annual Report of the Registrar General 1842*, p. 10.

[6] The proportion of women between 15 and 44 as a percentage of the total female population and the crude birth-rate were about the same for both countries during this period. See Connell, *op. cit.*, pp. 30, 37.

of marriage in Ireland was not typical of the period before 1841. Drake has examined the statistics for the 1830s and has concluded that a 'trend towards later marriage which they depict probably did not occur'.[1] Possibly at an even earlier period marriage took place at a lower age, but then the age at marriage would be *rising* throughout the early nineteenth century when population was increasing very rapidly. The only evidence for early marriage is literary rather than statistical, but if the evidence for the 1830s is typical we are unable to rely upon the estimates of casual observers. For example, Connell has written that 'according to an official summary of the immense mass of evidence presented to the Poor Inquiry Commission of 1836, men in Galway usually married when they were between 14 and 21; in Leitrim between 16 and 22; in Mayo and Sligo usually under 20, and in King's County between 17 and 20',[2] yet according to the 1841 Irish Census there were *only fifty-three* married men under the age of 17 in the whole of Ireland.[3] It is probable that the informants of the Commission had a vested interest in castigating the moral 'laxity' of agricultural labourers and small cultivators: they had to find an explanation for the poverty of the majority of the population, and what more convenient explanation than the Malthusian one?

Drake has argued that there are alternative explanations for the rapid expansion of the Irish population: (1) 'that a highly nutritious and regular diet of potatoes so improved the health of Irish women that their fecundity increased markedly';[4] and (2) 'that the universal acceptance of the potato as the staple food would lead to a once-and-for-all drop in the general level of mortality'.[5] There are two major difficulties with this interpretation: first that population increased rapidly only after 1772, whereas potatoes had been used widely in Ireland since at least the beginning of the eighteenth century; and second, that earlier diets were probably much more nutritious than the exclusive reliance on potatoes at a later date. Petty wrote in about 1671–2 that 'The Diet of these people [the Irish] is Milk, sweet and sower, thick and thin, which is also their Drink in Summertime, in Winter, Small-Beer or Water. . . . Their Food is Bread or Cakes, whereof a Penny serves

[1] Drake, *loc. cit.*, p. 311.

[2] K. H. Connell, 'Peasant Marriage in Ireland: its Structure and Development since the Famine', *Econ. Hist. Rev.*, 2nd ser., XIV (1961–2), p. 520.

[3] Population Census Ireland 1841, *Parl. Pap. 1843/24*, p. 439. There were only 480 married females under the age of 17.

[4] Drake, *loc. cit.*, p. 311.

[5] *Ibid.*, p. 312.

a Week for each; Potatoes from August till May, Mussels, Cockles and Oysters, near the Sea; Eggs and Butter, made very rancid, by keeping in Bags. As for Flesh . . . tis easier for them to have a Hen or Rabbit, than a piece of Beef of equal substance'.[1] Several contemporaries thought that the Irish poor could no longer afford milk and other 'extras' during the late eighteenth and early nineteenth centuries.[2] It seems inconceivable that the slightly more luxurious earlier diet was less nutritious than potatoes by themselves. And if potatoes were associated with higher fecundity, why were not Irish women more fertile than English women?

The death rate in Ireland appears to have been lower during the 1830s than it was in England. According to the retrospective statistics collected for the Irish census of 1841, the crude death rate was 16.8 per 1,000 for the years 1836–40,[3] whereas in England and Wales for the period 1838–41 it was 22.2 per 1,000.[4] That this finding is not an artefact of the method of collecting statistics or due to differences in the age composition of the two populations is demonstrated by comparing age-specific death rates for the year 1840/41.[5] Below the age of about 35 the Irish mortality rates were all lower than the English, but the great disparity occurred amongst young children—Ireland had a mortality rate of about 40 deaths per 1,000 children living under the age of five, whereas the equivalent English rate was about 67 per 1,000.[6] The explanation of this marked difference in child-mortality rates is probably that a much higher proportion of the Irish population lived in rural areas. Within Ireland, the urban civic districts had a child-mortality rate (about 78 per 1,000) well over twice that in the rural districts (about 35 per 1,000). The conclusion to be drawn from these comparisons is that like the age at marriage, and the age-specific birth and marriage rates, the age-specific death rate in Ireland was similar in

[1] Other writers during the late seventeenth century emphasize potatoes and milk in the diets of the Irish poor. See G. O'Brien, *The Economic History of Ireland in the 17th Century* (Dublin, 1919), pp. 137–42.

[2] G. O'Brien, *The Econ. Hist. of Ireland from the Union to the Famine* (1921), p. 21.

[3] *Ibid.*, p. 189.

[4] *Fifth Annual Report of the Registrar General 1843*, p. 379.

[5] As the number of deaths in 1840 was ascertained from a house to house survey made in the following year (1841), the figures presumably are reliable, especially for young children's deaths.

[6] For the Irish age-specific mortality rates see Connell, *Population of Ireland*, p. 193; for English mortality rates for roughly similar age groups, see Mitchell and Deane, *op. cit.*, pp. 38, 40; for the exact figures under the age of 5, *Fourth Annual Report of the Registrar General 1842*, p. 128.

about 1841 to that in England and Wales when allowance is made for distribution effects of population in urban and rural areas. This would imply that demographic factors were independent of economic differences, a conclusion similar to that reached from a study of the age at marriage and age-specific birth and marriage rates within England during the eighteenth and nineteenth centuries.

If the increase in Irish population is not to be explained in terms of a high birth rate associated with a low age at marriage, but in terms of a low death rate, what possible cause or series of causes could explain any *fall* in the death rate during the late eighteenth and early nineteenth centuries? We have already rejected the hypothesis that there was an improvement in the Irish diet during the eighteenth century. Professor Connell, after reviewing possible causes for a reduction in mortality, concluded that his 'examination of the social habits and the housing of the Irish, the dissemination of hospitals and dispensaries, the spread of vaccination and the incidence of fever does not support the proposition that in Ireland, as is said to have been the case in England, greater cleanliness and medical advances led to a substantial lowering of mortality'.[1] Professor Connell also reviewed the history of smallpox and inoculation, but unfortunately did not treat the subject at length; here it is only possible to elucidate some hypotheses and briefly illustrate them with relevant statistics.

Smallpox appears to have been present in Ireland at least from the Middle Ages onwards and had become endemic before the eighteenth century.[2] The disease seems to have occurred almost every year in Dublin during the period 1661–1746, when bills of mortality were kept.[3] According to statistics derived from these bills, smallpox accounted for about 20 per cent of the total deaths during the two periods 1661–90 and 1715–46.[4] Smallpox deaths 'accounted' for about 33 per cent of all children born during 1715–46, according to the Dublin bills of mortality. No other statistics of smallpox mortality are available for Ireland before the 1830s. However, several observers

[1] Connell, *op. cit.*, p. 239.

[2] As Rogers wrote in 1743: 'though of foreign Growth, and by Transplantation brought in amongst us, it is now become a Weed of our own Soil, and a Native of our Country'. Joseph Rogers, *Essay on Epidemic Diseases* (Dublin, 1734), p. 82.

[3] For a description of the content of the bills and relevant statistics, see J. Fleetwood, *History of Medicine in Ireland* (Dublin 1951), p. 65, and Dr. J. Rutty, *A Chronological History . . . of the Prevailing Diseases in Dublin* (Dublin, 1770).

[4] The actual figures are as follows: Dublin, 1661–90: smallpox deaths (annual average)—472, total deaths (annual average)—2,236. 1715–46 (excluding 1739): smallpox deaths—13,759, total deaths—74,585; total births—42,566.

described smallpox epidemics during one period of the eighteenth century. Dr. James Sims recounted the smallpox epidemic of 1766–7, writing that smallpox outbreaks 'with unheard of havock, desolated the close of this year [1766], and the succeeding spring of 1767. They had appeared above a year before along the eastern coast of the kingdom, and proceeded slowly westward with so even a pace, that a curious person might with ease have computed the rate of their progress. . . . As they had not visited the country for some years, numerous subjects were grown up for them to exercise their fury upon, and many blooming infants were just opening to the sun, in vain, since they were so soon to be cropt by this unfeeling spoiler. Of thousands who caught the infection in this [Tyrone] and the neighbouring counties, scarcely one-half escaped, and even of these, some with the loss of one or both eyes, and several with faces so altered, as to be known with difficulty by their most intimate acquaintances'.[1] A later epidemic in 1770 was less mortal but this was attributed to 'the want of subjects for them to exercise their fury upon, the preceding disorder having left few who had not undergone the malady, than to any abatement in their malignancy'.[2] These descriptions of smallpox epidemics in the countryside are identical with those to be found in England before the advent of inoculation, and smallpox was always more virulent in isolated country areas owing to a lack of a pool of antibodies.[3]

3

Inoculation was introduced into Ireland in 1725 and spread very slowly amongst the general population, although unfortunately little is known of the exact chronology. The watershed of the practice of inoculation in Ireland, like that in England, was probably the perfection of a safe technique by the Suttons during the 1760s. The Suttons appointed several partners in Ireland: 'Messrs. Houlton, Blake and Sparrow in Dublin; John Hailey, M.D. in Cork; John Morgan, M.D. in Straborne, Tyrone; and Messrs. Vachell, Ward, Shields & Arnold soon [1768] to be appointed to particular districts in Ireland'.[4] This development

[1] J. Sims, *Observations on Epidemic Disorders* (1773), pp. 36–38.
[2] *Ibid.*, pp. 134–5.
[3] See my paper in *Econ. Hist. Rev.*
[4] R. Houlton, *Indisputable Facts Relative To The Suttonian Art of Inoculation* (Dublin, 1768), p. 10.

appears to have marked the beginning of popular inoculation in Ireland. In 1769 'a special infirmary was set apart in the Foundling Hospital of Dublin, for Experimenting with inoculation upon the inmates'.[1] In April 1777 'agreeable to the humane resolutions of the King's County Infirmary, 461 persons were, in the course of last month, inoculated'.[2] The difficulty of tracing the history of inoculation in Ireland is that most of it was carried out by 'individuals [who] proceed from village to village several times during the year for the purpose of inoculating the infantile population',[3] a practice, of course, made necessary because there were at this time so few doctors in Ireland. Inoculation does not appear to have been used much during the 1766 epidemic as described by Sims, although he refers to the existence of 'inoculators' at that time.[4] Houlton observed in 1768 that several itinerant inoculators were claiming that they practised the safe Suttonian technique,[5] and as I have said this was probably the beginning of popular inoculation in Ireland.

By the beginning of the nineteenth century inoculation was practised almost universally. The Dublin College of Physicians, when asked in 1807 their opinion of vaccination, replied that 'Variolous Inoculation had been long, almost exclusively in the hands of a particular branch of the profession ("irregular practitioners") . . . being the usual medical attendants in families, and especially employed in the diseases of children. . . . Smallpox is rendered a much less formidable disease in Ireland by the frequency of inoculation for it . . . hence parents, not unnaturally, objected to the introduction of a new disease (vaccination) rather than not recur to that with the mildness and safety of which they are well acquainted.'[6] According to the Rev. H. Townsend, writing in 1810, the increase in population was partly due to 'the universal custom of inoculating children for the smallpox, a disorder, which was once a little less injurious in its ravages than the plague'.[7] The activities of the itinerant inoculators were noted in Derry in 1812,[8] and in Co. Water-

[1] Population Census 1851, *Parl. Pap. 1856/29*, p. 146.
[2] *Ibid.*, p. 422.
[3] Population Census Ireland 1841, *Parl. Pap. 1843/24*, p. XII.
[4] Sims, *op. cit.*, p. 42.
[5] Houlton, *op. cit.*, p. 25. 'Some, I am informed since my arrival in Ireland, are now travelling over several parts of the kingdom. . . .'
[6] *Report of the Royal College of Physicians of London on Vaccination* (1807).
[7] Rev. H. Townsend, *Statistical Survey of the County of Cork* (Dublin, 1810), p. 90.
[8] W. S. Mason, *Statistical Account, a Parochial Survey of Ireland*, I (Dublin 1814), p. 313.

ford, Cork, Kerry, and Clare at later dates.[1] Sir William Wilde noticed the activities of the inoculators as late as 1851.[2]

Connell accepts that inoculation was practised very extensively, but also accepts the traditional belief that inoculation spread smallpox to those who were not protected by it. I have dealt with this problem at some length elsewhere,[3] and it can be only briefly discussed here within the context of Irish experience. According to Sir William Wilde, vaccination was practised in Irish towns much more than in country areas, owing mainly to the preference for inoculation amongst the peasants.[4] Yet smallpox mortality was much less in the country areas than in the towns:[5]

Table 3. Irish Smallpox Mortality in Town and Country.

	Population (1841)	Smallpox deaths (1831–40)	Annual average smallpox deaths per million living
Civic districts	1,135,465	12,418	1,093
Rural districts	7,039,659	45,459	647

This difference cannot be explained by the different age structures of the town and countryside population—they were approximately similar—or by the greater extent of smallpox in the towns: everywhere in Ireland during the 1830s smallpox was a young child's disease, meaning that most children caught it (unless they were inoculated or vaccinated) by their fifth birthday.[6] In such a situation inoculation could not conceivably spread smallpox, as it was already a universal disease. Smallpox mortality was higher in urban areas because there was less inoculation and vaccination practised there; the rural areas had lower smallpox mortality rates because of the protection given by inoculation. The total smallpox mortality rate of Ireland was about 710 annual deaths per million living. Although this figure may appear at first sight to be high, it is, in fact, remarkably low if compared with earlier mortality rates. In Dublin during 1661–90, for instance, the smallpox

[1] *First Report of the General Board of Health in the City of Dublin*, pp. 94–97.
[2] Population Census Ireland 1851, *Parl. Pap. 1856/29*, p. 422.
[3] See the paper already cited.
[4] *The Epidemiological Society Report, 1852–53*, p. 29.
[5] *Royal Commission on Vaccination*, 1st Report (1889).
[6] 49,000 of the 58,000 total smallpox deaths during 1831–40 were of children under 5 years of age.

mortality rate had been about 8,600 per million.[1] Expressed as a proportion of total deaths, smallpox had accounted for about 20 per cent of deaths in the 1661–1745 period in Dublin, whereas in that city during 1831–40 it accounted for under 3 per cent of them.[2] The rate of 710 per million is also low by what might be expected if neither inoculation nor vaccination had been utilized on a wide scale. The case fatality rate of natural smallpox amongst infants was about forty deaths per 100 cases during the 1830s;[3] had all children under the age of 5 caught smallpox, the smallpox mortality rate would have been 400,000 deaths per 1,000,000 living rather than the 39,300 per 1,000,000 which was the actual rate for children under 5,[4] i.e. it would have been about ten times the actual rate.

The point of these hypothetical comparisons is to indicate the scale of saving of life by inoculation and vaccination. Although it is impossible to trace the exact decline of smallpox during the late eighteenth and early nineteenth centuries, there being no statistical information available for Ireland during this period, literary sources as already indicated suggest a rapid decline before the end of the eighteenth century. Sir William Wilde in his survey of smallpox epidemics mentions none after 1776, except for mild outbreaks in 1827 and afterwards.[5] Accepting, therefore, the effectiveness of inoculation, it may be concluded that the gradual disappearance of smallpox could account for the whole of the increase in population after about 1770.[6] The chronology of inoculation, it should be noticed, fits in very well with the great

[1] This is using Petty's population figure of 55,000 for Dublin; undoubtedly this is an underestimate, but so many deaths were not registered that the two underestimations appear to cancel each other out, i.e. the overall crude death-rate using Petty's population figure is about 40 per 1,000, a not unreasonable figure for a city the size of Dublin during this period.

[2] 'Report . . . by the . . . Vaccination Committee 1853', *Parl. Pap. 1852–53/101*, p. 80. None of the smallpox mortality statistics in this essay ought to be taken literally, as there were several reasons why smallpox deaths were under-registered.

[3] See the *Royal Commission on Vaccination*, 1st Report (1889), pp. 74, 215; *ibid.*, 6th Report, pp. 717–20; E. G. Edwards, *A Concise History of Smallpox and Vaccination* (1902), p. 55.

[4] Connell, *The Population of Ireland*, p. 219.

[5] Population Census Ireland 1851, *Parl. Pap. 1856/29*, p. 422.

[6] Certainly if the 1766–7 epidemic was typical of pre-inoculation experience, the disappearance of smallpox in Ireland could explain any increase in population. Generally, smallpox mortality appears to have been heavier in Ireland than in England; nevertheless population expansion in Ireland before 1770 was probably due to earlier long-term changes such as the disappearance of the plague. In this sense, the gradual elimination of smallpox would only explain the great acceleration of Irish population after 1770.

acceleration in population growth from about 1771 onwards as outlined by Professor Connell.[1]

4

I have suggested that the population growth in both England and Ireland during the latter half of the eighteenth and first half of the nineteenth centuries can be explained as a result of the gradual elimination of smallpox, and therefore may be considered independent of contemporary economic changes. But since it appears that the demographic experience of the two countries was very similar, why was it that economic effects were so different? The answer to this question is obviously complex, and involves consideration of a wide range of economic, social, and political factors; in my few remaining pages only some points of particular relevance can be suggested.

The cloth industry was England's chief commercial manufacture during the eighteenth century, but according to recent estimates it only accounted for about 5 per cent of the total national income,[2] and its domestic market appears to have hardly changed between 1695 and 1772.[3] As most of the expansion in the cloth industry before 1772 can be explained as a consequence of increasing exports, we must ask how much other economic growth during this period was due to domestic expansion. Deane and Cole have argued that a *general* economic expansion took place from the 1740s onwards. This conclusion is based, however, on an index of real output which is virtually an index of estimated population growth, as agriculture (43%) and rent and services (20%) are both based on questionable estimates of the size of population. An analysis of the production series that are available throws considerable doubt on the 1740s as a turning-point. As one writer has pointed out: 'Of the dozen or so commodities for which output figures are available there are several in which the levels reached in 1741–5 and 1746–50 were lower than those achieved earlier in the century. This is true of

[1] See Connell, *The Population of Ireland*, p. 25.

[2] P. Deane 'The Output of the British Woollen Industry in the Eighteenth Century', *Journal of Economic History*, XVII (1957), p. 221.

[3] According to Deane's estimate, domestic consumption of manufactured cloth was about £3 million in 1695. If one accepts the proportion of Yorkshire woollens and worsteds exported in 1772 as being typical of the country as a whole (at this time Yorkshire output accounted for about 60 per cent of the total), domestic consumption of manufactured woollen cloth was also about £3 million in 1772. See Deane, *op. cit.*, pp. 220, 221.

strong beer, starch, hides and skins, coal imports, raw silk and thrown silk. Indeed, for some of these commodities the 1740s is a low point. In other commodities, such as printed goods and soap, the acceleration of output was clearly later in the century.'[1] This criticism appears valid, since, if one takes Deane and Cole's own home industries index (beer, leather, candles, and soap), the uninterrupted and main increase in production certainly occurs after 1770.[2] One hypothesis which would explain differences in the chronology of increased consumption of different commodities is that the consumption of quality goods increased much sooner and in greater quantities than did that of cheaper goods. The output of tallow candles, used by poorer people, doubled between 1715 and the end of the century, whereas that of wax candles, used by the wealthier classes, increased nearly tenfold.[3] The production of high-quality white glass nearly quadrupled between 1747 and 1801, whereas that of common bottles only began to increase during the 1790s.[4] The best comparison between the output of quality and cheap

Table 4. Output of Quality and Cheap Goods 1695–1804.

	Imports of silk (1695–1704 = 100)	Strong beer production (1695–1704 = 100)
1695–1704	100	100
1705–14	92	99
1715–24	110	112
1725–34	130	104
1735–44	107	102
1745–54	116	108
1755–64	153	113
1765–74	182	112
1775–84	203	123
1785–94	225	136
1795–1804	217	163

[1] D. Whitehead, 'History to Scale? The British Economy in The Eighteenth Century', *Business Archives and History*, IV, No. 1 (Feb. 1964), p. 83.

[2] The index numbers were as follows (beginning at 1700 and continuing at every tenth year until 1800): 100, 98, 108, 105, 105, 107, 114, 114, 123, 137, 152. Deane and Cole, *op. cit.*, p. 78.

[3] T. S. Ashton, *An Economic History of England: The Eighteenth Century* (1955), p. 60.

[4] Mitchell and Deane, *op. cit.*, p. 267.

goods is between silk and beer.[1] The consumption of silk increased rapidly after 1755, whereas that of beer only really began to increase after 1775.

It may be suggested that the earlier expansion of the market for quality products was a result of the rapidly increasing population of the aristocracy, gentry, and other wealthy groups. During the eighteenth century about a quarter of the national income went to $3\frac{1}{2}$ per cent of all families, i.e. the aristocracy, gentry, and merchant class.[2] Due to decreased mortality their numbers probably quadrupled between 1750 and 1850,[3] and they were the social classes most able to translate their increased needs into effective demand. This could have occurred in several ways: by a switch from savings to consumption; by increased borrowing, including mortgaging of land; improvements of their assets, through the enclosure of land and a more intensive use of their capital in business;[4] and by a general exploitation of patronage through increased participation in Parliament, particularly with reference to finding places in the very rapidly expanding army.[5] The main problem would have been to find positions for their now surviving younger sons and provide their daughters with portions; possibly this was one of the reasons for the frequent failure of many of the poorer gentry and yeomanry during this period.

If the earlier analysis of the causes of the population increase is correct, mortality did not fall significantly amongst the poorer classes until after 1765, and this would explain why the consumption of cheaper commodities did not rise until after this date. It would appear that the domestic consumption of woollen cloth increased rapidly after about 1772: after this date the total output of woollen cloth rose, while the proportion exported fell from about 70 per cent in 1772 to 35 per cent

[1] Deane and Cole, *op. cit.*, p. 51. The index figures are only approximations.

[2] P. Mathias, 'The Social Structure in the Eighteenth Century: a Calculation by Joseph Massie', *Econ. Hist. Rev.*, 2nd ser., X (1957–8), pp. 42–45.

[3] See T. H. Hollingsworth, 'A Demographic Study of the British Ducal Families', *Population Studies*, XI (1957).

[4] Both the number of patents taken out and the number of bankruptcies increased sharply from the 1760s onwards: Mitchell and Deane, *op. cit.*, p. 268; Ashton, *op. cit.*, p. 254. The scale of possible profit from enclosures is indicated by the estimate of Gregory King in 1685 that only about half of the total land surface of England was cultivated, of which three-fifths was cultivated under the common-field system. See J. L. and B. Hammond, *The Village Labourer* (1919), p. 26 n. 1.

[5] According to an unpublished analysis of mine, the proportion of the old aristocracy in the House of Commons rose significantly during the eighteenth century, and younger sons of the aristocracy increased their numbers in the Church, Navy, and 'Civil Service', as well as in the Army. The colonial Army and mercantile 'administration' provided outlets particularly for younger sons of the gentry.

in 1805, and 20 per cent after the 1820s.[1] Beginning probably during the 1770s, there was a considerable expansion of the home market for cheap woollens and cottons, due almost certainly to an increase in population rather than a growth in *per capita* incomes. It is not necessary to describe the effects of the great upsurge in population after 1770 which affected every branch of economic and social life—the growth of canals, the improvement of roads, enclosure of land, development of the factory system—in short, the Industrial Revolution. Although increasing exports and the raised demand of the wealthy led to a growth of production, these were not the foundation of the change.[2] They helped to maintain the real incomes of the mass of the population, and therefore helped to translate increased needs (from an enlarged population) into effective demand, which raised prices and stimulated economic growth.[3] Only a radical expansion of mass markets could provide the sufficient condition necessary for the fundamental transformation of the economy, i.e. the growth of the new factory capitalism. It is no accident that this capitalism did emerge ultimately in Lancashire, after its earlier forms had developed elsewhere. Lancashire had been the centre of production of the very cheapest cloth in the early eighteenth century, and untrammelled by traditional constraints it was the natural place for the emergence of the factory system producing for a mass market.

5

In Ireland the result of the population explosion was the growth of a subsistence economy rather than an industrial revolution. Although the Irish census of 1841 returned about 30 per cent of the total occupied population as employed in industry, two-thirds of these were women, most of whom worked at home in domestic industry, providing goods for local consumption.[4] The only province with a sizeable male popu-

[1] Although this was partly due to the substitution of cottons for woollens in the export market, only about 30 per cent of all cottons were exported during the second half of the eighteenth century. See Deane and Cole, *op. cit.*, pp. 185, 196.

[2] The growth in the export market partly depended upon emigration, and thus on population increase at home; inoculation was also widely used in America and the West Indies, and so was contributory to population growth in these markets.

[3] According to figures computed by Arthur Young, the price of wheat (statute measure) at the Windsor Market was as follows: 1714–38—£1. 15*s.* 5*d.* per qtr.; 1739–63—£1. 14*s.* 2*d.*; 1764–88—£2. 6*s.* 6*d.* See A. Young, *Annals of Agriculture*, XIV (1790), pp. 228–30.

[4] T. W. Freeman, *Pre-Famine Ireland* (Manchester, 1957), pp. 76–77.

lation employed in industry was Ulster, the centre of the linen manufacture.[1] This industry had been encouraged since the beginning of the eighteenth century as a compensation for the destruction of the Irish woollen industry in 1699.[2] The export of linen cloth and yarn trebled between 1718–47 and 1748–77, about 90 per cent of it finding its way into the English market.[3] In 1771 it was estimated that the manufacture of linen was worth £2,200,106, 70 per cent of the output being exported.[4] Linen was estimated to be worth about half the total value of all exports during 1771–7,[5] but its export importance declined during the late eighteenth and early nineteenth centuries, while home consumption appears to have expanded sharply during the same period.[6] Cotton, however, began to displace linen, for, as one observer noted in 1840, 'men cannot live for what they get for [linen] weaving now. There is a great difference in respect of the appearance of weavers who come to market now and formerly; they are not so well dressed, nor near so comfortable looking: the fine sturdy young men, who once came to the market, have now gone out of the trade, and many have emigrated to America. I remember when it was the best trade in Ireland; now it has gone to nothing. The cotton trade has ruined the linen; formerly everybody wore linen, and now everybody wears cotton'.[7] The change was probably due to the abandonment of protection of Irish industry in 1825, as even the domestic cotton industry began to wilt under the competition from England.[8] The first cotton mill driven by water power in Ireland was established near Belfast in 1784;[9] by the 1830s and 1840s 'the deserted factory with its silent water wheel was already a familiar aspect of the Irish scene'.[10] One of the main reasons for the eclipse of Irish industry was the lack of coal, although presumably

[1] *Ibid.*

[2] Although the manufacture of woollen cloth was very small in Ireland at the end of the seventeenth century, it was growing very rapidly during the last decade. It was supressed at the instigation of English clothiers, who were afraid it might eventually provide overwhelming competition. See G. O'Brien, *The Economic History of Ireland in the 17th Century* (Dublin, 1919), pp. 227–9.

[3] A. W. Hutton, *Young's Tour of Ireland*, II (1892), pp. 200, 202.

[4] *Ibid.*, p. 201.

[5] *Ibid.*, p. 255.

[6] The following are contemporary estimates: linen manufacture 1771: exports —£1,541,200; home consumption—£658,906; value of linen manufacture 1817 —£3,151,752; exports of linen 1822—£861,944. See Hutton, *op. cit.*, p. 201, and O'Brien, *op. cit.* (1921), p. 302.

[7] Freeman, *op. cit.*, p. 85.

[8] See O'Brien, *Economic History of Ireland* . . . (1921), p. 311.

[9] Freeman, *op. cit.*, p. 85.

[10] *Ibid.*, p. 6.

the cheapness of labour might have more than offset the cost of importing coal from England.

Perhaps the failure of industry in Ireland was rooted in the nature of the country's social structure. Arthur Young had noted in 1779 that the 'only considerable manufacture in Ireland, which carries in all its parts the appearance of industry, is the linen; and it ought never to be forgotten that this is solely confined to the Protestant parts of the kingdom; yet we may see from the example of France and other countries that there is nothing in the Roman Catholic religion itself that is incompatible with manufacturing industry. The poor Catholics in the south of Ireland spin wool very generally, but the purchasers of their labour, and the whole worsted trade, is in the hands of the Quakers of Clonmell, Carrick, Bandon, etc. The fact is, the professors of that religion are under such discouragements that they cannot engage in any trade which requires both industry and capital. If they succeed and make a fortune, what are they to do with it? They can neither buy land, nor take a mortgage, nor even fine down the rent of a lease. Where is there a people in the world to be found industrious under such a circumstance?'[1]

Young was undoubtedly correct in emphasizing the lack of financial incentives for Catholics to engage in industry, and another factor probably as important was their lack of capital. Very little land was owned by Catholics, and as early as the late seventeenth century most of the Irish population were peasants relying on subsistence farming. According to one observer writing in 1691, 'their food is mostly milk and potatoes, their cloathing coarse bandrel cloth and linen, both of their own make; a pot of gruel; a griddle whereon to bake their bread, a little salt, snuff, and iron for their ploughs being almost all they troubled their shopkeeper or merchant for. A little hut or cabin to live in is all that the poverty of this sort hope or have ambition for'.[2] Petty had estimated that out of a total of 200,000 houses, 160,000 were without any chimney, suggesting that they 'live in a brutish nasty condition as in cabins with neither chimney, door, stair nor window'.[3] With this degree of poverty it must have been impossible for Catholic peasants to acquire capital sufficient to establish manufacturing industry, quite apart from the lack of a home market suitable for the absorption of such manufactures. Any capital available was owned by the Protestant

[1] Hutton, *op. cit.*, p. 65.
[2] O'Brien, *op. cit.* (1919), p. 141.
[3] *Ibid.*, pp. 137–8.

landlords, many of whom were absentees; and as the population grew it became increasingly lucrative for them to invest their money in land, from which it was possible to obtain very high rents.[1] The derivation of these rents was described by Arthur Young: 'The poverty, common among the small occupying tenantry, may be pretty well ascertained from their general conduct in hiring a farm . . . they provide labour, which in England is so considerable an article by assigning portions of land to cottars for their potatoe gardens, and keeping one or two cows for each of them, and by means of living themselves in the very poorest manner, and converting every pig, fowl, and even eggs into cash, they will make up their rent . . .'[2]

In 1841 Ireland had a subsistence economy based on small peasant cultivation, widely scattered throughout the whole country: only about 20 per cent of the population lived in villages and towns, the rest in isolated cabins.[3] Pressure of population drove cultivation of potatoes 'towards the summits of the hills'[4] and meant that 'every possible spot of land is laboured'.[5] Subdivision of land and an almost exclusive potato diet enabled population to grow, inasmuch as the survivors of diminished mortality did not starve—until the subsistence economy collapsed and there occurred the great famine. The causes and consequences can best be seen in the following table:[6]

Table 5. Size of Land Holdings in Ireland, 1841 and 1851.

Size of holdings	Number of holdings	
	1841	1851
Not exceeding 1 acre	134,314	37,728
Exceeding 1 but not exceeding 5 acres	310,436	88,083
Exceeding 5 but not exceeding 15 acres	252,799	191,854
Exceeding 15 but not exceeding 30 acres	79,342	141,311
Exceeding 30 acres	48,625	149,090

The very small peasants and casual labourers were virtually eliminated within a decade: these were the inhabitants of the 'growth class' hous-

[1] See O'Brien (1921), pp. 12, 89, 97, 98.
[2] Young, *op. cit.*, pp. 31, 32.
[3] Freeman, *op. cit.*, p. 27.
[4] Connell, *op. cit.*, p. 96.
[5] *Ibid.*, p. 118.
[6] O'Brien, *op. cit.* (1921), p. 59.

ing—one-room cabins—which declined in number by 355,689 between 1841 and 1851, a decline of about 70 per cent.[1] The majority of the people leaving these cabins probably emigrated, although their reliance on potato subsistence meant that many of them starved to death or died from fevers associated with the famine.

6

Unable to industrialize, and with a rapidly expanding population increasingly dependent on the potato, famine in Ireland was inevitable. In England, on the other hand, all the conditions for industrial growth had been present before the population explosion: a relatively high standard of living and a social structure encouraging enterprise and providing a potential mass market; a thriving textiles industry; the existence of provincial capital markets and a great and growing commercial centre in London; relative political stability; a progressive agriculture; sufficient technical innovation; abundant market outlets and sources of supply in overseas markets—to mention only the best known of the much-discussed influences on growth. Although in both countries population increased rapidly through the use of inoculation against smallpox, England was fortunate in being able to industralize and thus avoid the mass starvation that was the disastrous fate of Ireland.

[1] *Ibid.*, p. 59.

5

P. E. RAZZELL

Statistics and English Historical Sociology

I

Interesting examples of the use of statistics in studying historical sociology are to be found in the recent work of the Cambridge Group. Laslett has presented evidence to show that the nuclear family was the basic form of family structure in seventeenth century England, a finding which contradicts the conventional sociological generalization about industrialization destroying the extended family.[1] Similarly, Wrigley has published statistics of pre-marital conception rates in Colyton, Devon during the period from the late sixteenth century to the middle of the nineteenth,[2] which can be used to test generalizations about sexual habits in pre-industrial society and how they change over time. Wrigley found that the proportion of children conceived before marriage in Colyton had been 30 and 40 per cent during the seventeenth and eighteenth centuries, and had risen to over 50 per cent by the early nineteenth.[3] This type of evidence casts doubt on the popular sociological notion that pre-marital sexual relationships are of recent origin. It also contradicts the social historians' picture of the early nineteenth century as being a transitional period in the establishment of Victorian morality.

The major problem in the use of statistical data in the study of historical sociology is the unreliability of much of the evidence which forms the basis of the data. Laslett and Wrigley have both used original records in such a way as to be confident that their findings are reliable. In this essay I wish to illustrate the use of certain types of statistical sources which have been more or less

[1] P. Laslett, *The World We Have Lost* (1965), pp. 91–2. For a confirmation of this conclusion see the *1851 Census*, Vol. 1, Table 1, p. xliii.

[2] E. Wrigley, 'Family Limitation in Pre-Industrial England', *Economic History Review*, 2nd Series, Vol. XIX, No. 1, April 1966.

[3] A study recently published in *Population Studies* (Nov. 1966) showed that in a sample of 77 parishes the pre-marital conception rate was at least 20 per cent during the seventeenth century, rising to over 40 per cent during the eighteenth.

neglected, but are sufficiently reliable to test sociological hypotheses about English history. None of the findings presented are meant to be conclusive but are intended as illustrations of the way these sources may be used.

A much neglected source has been the marriage registers compiled after the introduction of civil registration. The following information was provided on each marriage certificate: (a) occupation of groom; (b) occupations of fathers of both groom and bride; (c) whether the groom and/or bride were able to sign their own names or not; (d) streets or places in which marriage partners were residing (sometimes); (e) age at marriage (sometimes). Using this type of information, a pilot study was carried out for All Saints Parish, Maidstone, for the period 1837–38,[1] and I shall briefly describe some of the sociological results of this study.

Two main subjects may be studied by using information from marriage certificates: social class differences and social mobility. The major problem in studying both subjects is how to establish criteria and define appropriate social classes, but it was possible to partially solve this problem by using some of the distinctions made in the register itself. Of a total of 115 grooms whose occupations were noted, 45 were registered as labourers, who tended to be a distinct and homogeneous sociological group, e.g. 17 of the 20 grooms who were living in Stone Street at the time of their marriage were labourers. The tendency for labourers to live in the same areas of the parish is confirmed by information from the 1841 census tracts for the town: both agricultural labourers and unskilled labourers working in the local paper-making factory and elsewhere tended to concentrate in special geographical clusters. The geographical distribution of different occupational groups is naturally quite complex in detail, with a general tendency towards overlapping. Some labourers lived in the same streets as skilled journeymen artisans (and occasionally with people of higher occupational status), who in their turn sometimes resided in the same streets as master artisans, tradesmen and professional people in other 'fringe' areas. However, the fact that 85 per cent of all grooms registered as residing in Stone Street (according to the Marriage Register) were labourers, indicates a sufficiently high concentration to treat labourers as a distinct residential group. They were also a relatively homogeneous group

[1] This marriage register is lodged in All Saints Church, Maidstone (Kent).

with respect to education; 22 of the 45 grooms who were labourers were unable to sign their own names in the marriage register, whereas this was true for only five of the 70 remaining grooms. This social class difference in education was also reflected in differences between different types of bride: 34 of the 45 brides marrying labourers were unable to sign their names, compared with only 13 of the remaining 70.

Although I have used geographical residence and education as criteria for defining social class, it would be technically more accurate to use them as criteria for what Weber called status groups, unless they were determining factors in the formation of the occupational groups (social classes are defined as essentially economic power groups). Weber's conception of the relationship between social classes and status groups was very complex, so I will attempt to briefly summarize in simplified form the apparent relationship implicit in his writings. Status groups may be seen as the social 'routinization' and stabilization of the much more dynamic and changing social classes; the stratification of status groups and social classes is likely to be identical during a historical period of little economic and social change (such as the European Middle Ages). Using a Weberian scheme of 'ideal type' analysis, we may say that during such a period there is a high degree of social homogeneity within social classes and a very insignificant amount of social mobility or exogamy between them. In order to test whether the labourers in Maidstone constituted a status group according to these criteria, it is not sufficient to know that they formed a relatively homogeneous group with reference to education and geographical residence, but it is also necessary to analyse the pattern of social mobility into and out of this class as well as the degree of endogamy practised.

In the Maidstone sample, 37 of the 45 grooms who were labourers were themselves sons of labourers, while 8 sons of 45 labourers had a different occupation from their fathers, indicating little social mobility into or out of this occupational group. This conclusion is confirmed by the fact that sons and daughters of labourers had approximately similar illiteracy rates as grooms who were labourers and their brides, i.e. education was a function of social class and not a factor fostering social mobility. There is information available in the Maidstone sample on the occupations of fathers of 44 brides who were married to labourers: 29 of these

fathers were labourers themselves. As labourers only formed about 39 per cent of the sample of fathers, the proportion of labourers' brides marrying sons of labourers (about 66 per cent) is significantly greater than would be expected if marriage occurred randomly amongst the occupational groups, i.e. there was a relatively endogamous pattern of marriage amonst the Maidstone labourers. This occupational group formed a status group, in that it was characterized by a similar area of geographical residence, low social mobility and relative endogamy. Also the 'style of life' of labourers was distinctive in that they were illiterate to a much greater extent than other occupational groups.

The sociological distinction between unskilled labourers and skilled artisans is an important one for interpreting English social history. Contemporary observers such as Francis Place were aware of its importance for understanding differences in 'moral' attitudes and style of life. Henry Mayhew believed that 'the transition from the artisan to the labourer . . . is so great, that it seems as if we were in a new land, and among another race'. He believed that the difference between the two groups was significant in all respects, including politics, with the artisans being 'red-hot' radicals and the unskilled labourers being either apathetically unpolitical or for the 'maintenance of things as they are'. He cited the example of the operative tailors among whom 'there appeared to be a general bias towards the six points of the Charter' which contrasted markedly with the coal-whippers who 'were extremely proud of their having turned out to a man on the 10th of April, 1848, and become special constables for the maintenance of law and order on the day of the great Chartist demonstration'.[1] Hobsbawm has recently emphasized this distinction in his discussion of the labouring aristocracy. Skilled artisans received twice the wages of unskilled labourers, and were sufficiently respectable to merit the appellation of 'lower-middle class' on certain occasions.[2] The association between the lower-middle class of artisans and small tradesmen and puritanism, with all that it implied for political radicalism, was strong as early as the seventeenth century.[3] The linking of artisans with small tradesmen was recognized as valid by the Registrar-General in 1838 when

[1] H. Mayhew, *London Labour and the London Poor* (1861), Vol. 3, p. 233.
[2] E. J. Hobsbawm, *Labouring Men* (1964), pp. 273–4.
[3] The best statistical evidence for this conclusion is to be found in W. A. Cole, *The Quakers and Politics 1652–1660* (University of Cambridge Thesis 1955), pp. 302–318.

statistics of suicide rates in London were published: labourers—2·9 suicides per 10,000 males (20 years and above) living; artisans and tradespeople—6·0 per 10,000.[1] This emphasis on the distinction between artisans and labourers does not mean that it was the most important class difference during this period and it is clear that other equally valid distinctions can be made, e.g. Mayhew also noted the marked income and educational differences between 'society' (trade union) artisans and those working in a ruthlessly competitive situation under the 'sweated' conditions of their own garrets. E. P. Thompson has recently argued that a new type of working class solidarity, cutting across manual occupational boundaries, emerged during the first half of the nineteenth century. A more revealing analysis of class structure is that made by Foster in his recent work,[2] which is based on a modified Marxist theoretical framework. Foster has made a distinction between Oldham with an economy dominated by a small number of very big firms, and Northampton where there were a large number of small firms. In Oldham the social distance between skilled and unskilled workers was small whereas in Northampton it was significantly greater. Foster has measured social distance by using the indices of inter-marriage and neighbourhood residence patterns, statistics of which he has compiled from local marriage registers and census documents. He has also linked the structure of status groups with the nature of class consciousness and conflict groups (what Weber called 'Party'), although there are formidable methodological problems involved in measuring 'class consciousness'.

A thorough analysis of the social structure of Maidstone would involve a systematic analysis of social mobility, inter-marriage, educational and neighbourhood residence patterns by occupational group, linked with other appropriate evidence about styles of life, as well as political activity. The latter type of evidence is almost certainly going to be of a literary kind, except where poll-book information is available (this is likely to be rare for groups such as labourers).[3] There is the additional difficulty of being unable to distinguish from census records (and the like) real sociological

[1] *3rd Annual Registrar-General's Report*, 1841, p. 79.
[2] In H. J. Dyos (Ed.) *The Study of Urban History* (1968).
[3] Since this article was written Vincent has written his book *Pollbooks: How Victorians Voted* (1967) which shows that Maidstone labourers voted consistently more Conservative than did craftsmen.

differences between occupations which are listed in identical manner but may in fact be very different, e.g. a 'tailor' may be a master employing several men (the 1851 census was supposed to have noted this but did not always do so), a skilled journeyman working in a superior workshop (Mayhew's 'society' man), a semi-skilled member of a tailoring sweat-shop, or a garret-master working under 'sweated' domestic conditions. The incomes of these groups are known to have been very different, and it is questionable whether they ought to be put together in the same class category.

In the analysis of the Maidstone data, I have restricted the discussion to the sociological differences between labourers and other occupational groups; this is mainly due to the nature of the data itself, i.e. the social homogeneity of the Maidstone labourers became clear from even a cursory examination of the statistical evidence, which was not true for other occupations. It is possible, however, to assess to some extent the social mobility pattern for the total Maidstone sample. Of 115 grooms, 65 had the same occupations as their fathers, while a further 11 had the same occupations as their fathers-in-law. It is difficult to measure total social mobility for this group, as there are no readily available criteria to distinguish the social status of the different occupations. A somewhat arbitrary method is to divide the sample of grooms into two equal groups: 57 unskilled as against 58 skilled and others. The unskilled includes all the labourers plus 6 servants, 4 bricklayers and 2 watermen, while the skilled includes all the artisans (such as papermakers and carpenters), tradesmen and professional people, as well as one or two dubious cases such as army privates. Support for this division is provided by the fact that the occupations of the non-labouring grooms who were illiterate were: one servant, bricklayer, waterman, army private and basket-maker (a total of 5 cases). On this basis of social division of occupations, of the total 115 cases, 5 grooms achieved a 'higher' position than their fathers, as against 16 whose occupational status was 'lower'. This result is not surprising during a period of rapid population increase in an area outside of industrial expansion, where most upward social mobility took place. Any index of total social mobility, e.g. 18½ per cent of men crossing the two social classes, would be misleading as the basis of comparison with more recent experience, because of differences in

social structure and the questionable validity of such an index.[1] A more appropriate comparison is that for specific occupational groups: about 82 per cent of the groom labourers in Maidstone were the sons of labourers, whereas the comparable figure for the cohort of unskilled occupations in Glass's twentieth century sample was about 40 per cent.[2] This suggests a significant increase in social mobility but is only suggestive because the two samples are not directly comparable. It is clear, however, that social mobility must have been low during the earlier period, which is associated with the high proportion of Maidstone sons who followed their fathers' occupations.

It is possible that there was a greater amount of social mobility in areas other than Maidstone—particularly in industrial regions—and during the pre-industrial period before the economic polarization associated with capitalism had developed. Richard Baxter in his book on the *Poor Husbandman* written during the latter part of the seventeenth century, noted how easy it was for agricultural labourers to set themselves up as small tenant farmers, although the economic and social benefits from this step do not appear to have been large. It is well known how relatively easy it was for journeymen weavers to set themselves up as small independent clothiers in areas such as Yorkshire before the emergence of the capitalist factory system. It is obviously desirable that such forms of social mobility be statistically measured, but unfortunately there is a great paucity of reliable information. One possible source is the Anglican marriage licences which sometimes give the occupations of both grooms and their fathers. The Sussex marriage licences for the period 1755–1800 are particularly good for the information they give; of 60 cases sampled, 44 fathers and sons were listed as having the same occupations.[3] The proportion of sons and fathers having the same occupation was slightly higher in this Sussex sample than it is in that from Maidstone. This suggests that there was no significant amount of social mobility in rural areas during the pre-industrial period, although it is possible that the enclosure movement, etc., had affected

[1] Lipset and Bendix used such an index in their comparative study of social mobility in industrial societies; their index is particularly questionable as it does not allow for distinction between upward and downward social mobility. See S. M. Lipset and R. Bendix, *Social Mobility in Industrial Society* (1959), pp. 25, 26, 72.

[2] D. V. Glass (ed.), *Social Mobility in Britain* (1953), p. 187.

[3] D. Macleod (ed.), Calendar of Sussex Marriage Licences, *Sussex Record Society*, Vols. XXXII and XXXV.

Sussex sufficiently by 1755–1800 to diminish the kind of mobility described by Baxter. It is also possible that intra-generational social mobility was much more frequent than the form of inter-generational mobility (between fathers and sons) that we have been measuring. There is no evidence available on this for the earlier period, and only a fragment of information for the mid-nineteenth century. Williams has studied the census records of the West Country village of Ashworthy for the years 1841 and 1851.[1] According to his published statistics, in a village of a population just over 1,100, only two men who were labourers in 1841 were farmers by 1851, whereas two families whose heads were farmers in 1841 had become paupers by 1851 (the heads of the families dying in the intervening period).[2] This finding indicates little intra-generational social mobility, which confirms the other statistical evidence which we have considered for the period before the mid-nineteenth century.

The discussion of sociological statistics in the study of English history has been mainly confined in this paper to evidence derived from marriage registers kept under the civil registration system and lists of special marriage licences issued by the Anglican Church. This raises the question as to how accurate these marriage records were with reference to the sociological information contained in them. It is partially possible to check the accuracy of the Anglican special licences by comparing some of their information with that in parish registers (this is also a cross-check on the reliability of the parish register). Some of the Sussex licences give the period of residence in the parish from which a person was married. In the cases where this was 'all his (or her) lifetime', it is possible to check back in the parish register to see whether they were actually born in the parish, and whether the age at marriage given in the licences is accurate. This was done for 40 persons married by licence issued in the Chichester Archdeaconry during the period 1760–1800.[3] Only two of these 40 persons could not be traced in the parish register, no mention being made of their family during the estimated period of their births. Thus both the parish register and the marriage licences are relatively accurate as records in respect to when and where a person was born and how

[1] W. M. Williams, *A West Country Village Ashworthy* (1963).
[2] Ibid., p. 128.
[3] Macleod, op. cit.

long they had lived in the parish before marriage. The ages at marriage are somewhat less in agreement in the comparison between parish register and marriage licence. Of the 38 traced cases, there was approximate agreement in 22, a difference of about one year in nine, and somewhat greater differences in the remaining seven cases. All but one of the differences were due to the understatement of age in the marriage licences, but such differences are not large enough to significantly affect median ages at marriage calculated from the two types of data (age stated in the licences and reconstituted age at marriage from the parish register).

The information in the Sussex licences enables us to compile statistics of the relative geographical mobility of different occupational groups.

Table 1

PROPORTION OF PEOPLE LIVING IN SUSSEX PARISHES FOR ALL THEIR LIFE BEFORE MARRIAGE[1]

Groom's Occupation	*Period*	*Per cent 'All Their Lives'*			
		Grooms %	*N*	*Brides* %	*N*
Labourers	1786–1800	2	100	18	100
All Occupations	1793–1794	16	100	24	100
Farmers and Yeomen	1790–1797	46	100	39	100

The variations in geographical mobility were much greater amongst grooms than brides. The difference between labourers and farmers was most marked: two as against 46 per cent living all their lives until marriage in their parish of birth. This result should not surprise us, for most farmers and yeomen (as opposed to 'husbandmen') probably owned some of their own land which would tend to tie them to particular parishes, whereas labourers owning no land had to move to areas where cottages and remunerative work was available. This is reflected in literary evidence, e.g. the description of the hire at local farms of labourers for the year. Presumably women were more likely to live all their lives until marriage in their parish of birth as there was less economic necessity for them to move, although this was not true of domestic servants.

[1] Ibid.

The occupational differences in geographical mobility have been studied by Williams in his analysis of the 1841 and 1851 census records of Ashworthy. He has studied both immigration and emigration from the parish during this decade.

Table 2

INTERCENSAL MOVEMENT OF (ASHWORTHY) POPULATION 1841–1851[1]

	Farmers' Families	*Craftsmen's Families*	*Labourers' Families*	*Other*
IMMIGRATION				
Living in same dwelling	161	76	115	66
Moved within Ashworthy	42	32	102	34
Came to Ashworthy	19	27	123	38
Changed status	10	—	—	—
Children born in Ashworthy	71	55	101	23
Total 1851	303	190	441	161
EMIGRATION				
Living in Ashworthy (1841 and 1851)	213	108	317	
Probably moved from Ashworthy	64	31	202	
Died	21	11	73	
Changed status	9	—	—	
Not known	—	11	36	
Total 1841	307	161	628	

These statistics confirm our conclusions that farmers were very much less geographically mobile than labourers, and this was true even for movement within the parish itself. However, it is possible to produce statistics for other parishes to show that labourers had lower mobility rates than the general population,[2] and this is a subject that can only be settled after very much more research.

The statistics of geographical mobility so far considered suggest that the traditional picture of stable English village communities in which inhabitants lived their whole lives, is incorrect. This point is sociologically important as sociologists have too easily assumed that the pre-industrial English village formed a 'Gemeinschaft' type of community, with the sense of community based on

[1] Williams, op. cit., p. 128.

[2] For example, in Harlow, Essex (1851), labourers formed about a third of the resident natives but only about a fifth of people not born in the parish.

life-long face-to-face social contact within the context of a closed system of social relationships. The 'Gemeinschaft' community arises when sociability is structured between individuals for a major part of their lifetime (in the extreme case for the whole of their lifetime). One way of studying this subject is to examine the proportion of any village population which has lived in that village since birth. According to Williams's statistics for Ashworthy, just over 72 per cent of the 1851 population were born in the village;[1] this figure includes children as well as adults, which would tend to be higher than the proportion just for adults. This is reflected in Williams's findings, for amongst adults 67·8 per cent of farmers, 59·6 per cent of male farm workers and servants and 62·5 per cent of female farm servants were born in Ashworthy.[2] These proportions are still surprisingly high, in the light of the movements of the population into the village between 1841 and 1851; if we exclude children born in the parish during this decade, about a quarter of the 1851 population had moved into the village during the ten-year period. It is therefore surprising that such a high proportion of adults were listed as having been born in the village, although there is no necessary contradiction between the two types of evidence. It is possible that many families (particularly those of labourers) moved to several parishes before coming back to their home parish. Evidence for this is to be found in the 1851 census documents, e.g. Jonathan Foster, a labourer, was born in Latton, Essex, and his wife Sarah was born in Harlow, Essex; their first five listed children were born in Latton, but the last two were born in Harlow, where the whole family was enumerated in 1851. Much of the migration into and out of Ashworthy might have been of this type and would explain the high proportion of people listed as having been born in the parish. Another factor of some importance explaining the discrepancy between the statistics of migration and 'nativity' is the greater number of emigrants than immigrants—much of the geographically mobile population found its way into large towns rather than other villages, thus diminishing the proportion of 'foreigners' in any one village. It is therefore possible that there was more geographical mobility between villages (and therefore lower proportions of native populations in these villages) during

[1] Williams, op. cit., p. 123.
[2] Ibid.

the pre-industrial period and in fact this does seem to have been the case, e.g. of the 401 people living in Clayworth in 1676, only 158 were still living there in 1688, 91 dying in the parish during the intermediary period.[1] However, other types of evidence suggest that there was no significant increase in geographical mobility, e.g. see Table 3.

Table 3

PROPORTION OF PEOPLE MARRYING IN COLYTON, DEVON, WHO WERE BORN IN THE PARISH[2]

Period	*Number of Marriages*	*Number married and born in the parish*		*Per cent*	
		Men	*Women*	*Men*	*Women*
1560–1646	854	258	371	30	43
1647–1719	379	109	136	29	36
1720–1769	424	90	104	21	25
1770–1837	888	219	275	25	31

Although the English rural population was geographically mobile as early as the late sixteenth century, most of this mobility was probably restricted to a group of local parishes. There is no systematic statistical evidence for this conclusion for the earlier period, and only a limited amount for the later one. According to the 1841 population census, 80·7 per cent of the English population were born in the county that they were living in at the time of the census, and Williams concluded from his study of the 1851 census records of Ashworthy that most of the immigrants into the village were born within the area of a ten-mile radius of the parish.[3] This confirms what we know about the area in which migration occurred from the study of settlement certificates, as well as conclusions reached from an examination of particular family histories, e.g. the surname Dilnot was confined to a group of East Kent parishes, within a circle of a 20 mile radius, from as early as the fourteenth century through to the nineteenth.[4]

Not only was the rural population very much more mobile than has been commonly assumed, but the inhabitants of large towns seem to have moved very frequently from one house to another within the town itself.

[1] P. Laslett and J. Harrison, 'Clayworth and Coganhoe', in H. E. Bell and R. L. Ollard (eds.), *Historical Essays 1600–1750* (1963), p. 174.
[2] Wrigley, op. cit.
[3] Williams, op. cit., p. 123.
[4] I am grateful to Mr R. Dilnot for this information.

Table 4

LENGTH OF TIME WHICH THE HEADS OF FAMILIES HAVE RESIDED IN THEIR PRESENT DWELLINGS

(St. George's-in-the-East, 1848)[1]

	Families	*Single Men*	*Single Women*	*Total Families*
1–4 weeks	60	3	2	65
1–6 months	369	10	12	391
6 months–1 year	270	17	13	300
1–3 years	467	18	12	497
3–6 years	269	8	6	283
6–9 years	148	3	—	151
9–12 years	69	—	4	73
Over 12 years	136	2	7	145
Not ascertained	14	27	8	49
	1,802	88	64	1,954

This table summarizes a survey conducted by the Royal Statistical Society amongst the poor of St. George's-in-the-East, London, in 1848. The median period of residence for all families was about two years, a very short period of time compared to the lengthy periods spent in particular houses according to current surveys of working class populations such as that in Bethnal Green. In fact it is possible to make some kind of comparison of geographical mobility patterns in Bethnal Green at the middle of the nineteenth and twentieth centuries. According to a survey carried out by Glass and Frankel in 1944 seventy-seven per cent of the heads of families were born in the borough of Bethnal Green; a sample of 100 adults residing in Temple Street, Bethnal Green, in 1851 had an equivalent figure of 25 per cent, i.e. 25 of them had been born in the parish. Of course these figures are not strictly comparable, but they probably suggest the significant difference between the two periods fairly accurately, and indicate the kind of historical comparisons that can be made with this type of data.[2]

The reasons for the high amounts of geographical mobility within places like St. George's-in-the-East and Bethnal Green during the mid-nineteenth century are not hard to find. Their

[1] See the *Journal of the Royal Statistical Society*, Vol. XI (1848).

[2] One working-class informant in the early 1950s could not remember anyone moving into the street of seventy houses in which he lived (in Bethnal Green) during a forty-year period. See J. H. Robb, *Working Class Anti-Semite* (1954), p. 57, for this and other information about geographical mobility in Bethnal Green.

total population was expanding very rapidly during the nineteenth century, e.g. the population of the borough of Bethnal Green multiplied by about six times during the second quarter of the nineteenth century[1]—as the indigenous population was expanding at a very much slower rate, most of the increase came from immigration into the area. It is possible that other factors played a part in the very high turnover in house occupation in a place like St. George's-in-the-East: the need to move near new employment (transport being very inadequate) or the frequent evictions of the poor from their homes because of their inability to always pay the rent (this latter factor may have played a major part in the village of Ashworthy where the other factors are not likely to have played such an important part). Much of the mobility so far discussed took place within a relatively small area so that many of the immigrants into Temple Street, Bethnal Green, for example, came from neighbouring parishes of Shoreditch and St. Lukes. However, many of the sample came from outside London, a fact which is also reflected in the statistics of 'nativity' for London as a whole: of the 1·4 million adults living in London in 1851, about a half had been born outside the city. In fact this is a relatively high proportion compared with the relevant statistics of other towns during the same period: of Manchester and Salford's adult population of 226 thousand only just over one quarter were born in the city. Even very small towns like Dorchester, Dorset (adult pop. 3,734), Truro, Cornwall (adult pop. 6,161) and Bedford (adult pop. 6,354) had very low proportions of resident adult natives: 32 per cent, 38 per cent and 28 per cent. A place like Birmingham with an adult population of 127 thousand in 1851 had a higher proportion of natives: 44 per cent.[2] This proportion was higher than that found in some small villages, e.g. the parish of Havering, Essex (adult pop. 233), had only 11 per cent adult native residents.[3] Mere size was not the only factor in determining the proportion of native residents; the economy of a particular town, the demand for labour from the countryside, etc., would all determine the pattern of geographical mobility. Havering, Essex, probably had such a small proportion of native residents because it was so near London, which drew

[1] Ibid., p. 195. According to census data the population of Bethnal Green district quadrupled between 1801 and 1851.

[2] For all these statistics of nativity see *1851 Census*, Vol. 1, Population Tables 2, p. 418.

[3] See the 1851 Census documents for Havering in the Public Record Office.

much of its population from the surrounding countryside. London itself was big enough to provide work for all its native residents, who might have to move from parish to parish, but would still be able to find work and housing somewhere within the city. Villages relatively isolated from large towns appear to have had a large proportion of resident natives, e.g. of Garsington, Oxon's 327 adult population, 70 per cent had been born in the village according to the 1851 Census.[1] Garsington is thus like Ashworthy in its high proportion of native residents; an example of an 'intermediate' village is Harlow, Essex, of whose 275 adult population (1851), 39 per cent had been born there.[2]

It is clear that we cannot assume that agricultural villages were necessarily Gemeinschaft villages—many had relatively mobile populations who had not shared socially structured relationships for the major part of their lifetimes. This provisional conclusion is logically related to findings about the structure of the family and the relationship between neighbourhood and kinship. Willmott and Young found that the 'extended family' is common in traditional Bethnal Green, but this is the case only because the population is so static. If there is little migration into or out of a community, a network of kinship relationships is bound to be built up (unless the population is decreasing rapidly). This may be illustrated by the case of Garsington where the same surname exists much more frequently than it does in Maidstone during the same period; of a sample of 100 listed names of heads of households taken from the 1851 Census in both places, the most frequent name (Quartermaine) was mentioned nine times in Garsington as against only three (King) in Maidstone. This is what one would expect as the former place had 70 per cent resident adult natives while the latter had only 36 per cent.[3] This is not the only factor determining kinship neighbourhood patterns, as even if a population was geographically mobile it could still migrate with other members of the family.

It is partly possible to measure the geographical mobility patterns of family members from information in the Sussex licences. When a person getting married was under 21 they needed their parents' consent and the places of residence of child and parent were given. Of 100 grooms, 42 were residing (during the

[1] See the 1851 Census document for Garsington in the Public Record Office.
[2] 1851 Census documents for Harlow in the Public Record Office.
[3] *1851 Census*, Vol. 1, Population Tables 2, p. clxxiii.

latter half of the eighteenth century) in the same parish as their parents at the time of marriage; the comparable figure for brides is 80 out of 100. Thus brides were much more likely than grooms to live in the same parish as their parents, although they may have subsequently moved more frequently to their husband's present parish where he presumably worked. There were significant differences amongst different occupational groups for the grooms: all 12 farmers and yeomen in the sample lived in the same parish as their parents, whereas this was true for only four of 24 labourers, none of 28 husbandmen (tenant farmers), but as many as eight of ten artisans.[1] These findings confirm those about class differentials in geographical mobility for the Sussex sample and suggest that economic factors were most important in determining the relationship between kinship and neighbourhood. The whole question of neighbourhood and kinship patterns is clearly very complex, as is the related theme of geographical mobility. Only after much further research into community and class differences will it be possible to make confident generalizations. What is certain is the influence of population and economic growth on the mobility and kinship neighbourhood patterns. It was possible for kin to cluster in the same neighbourhoods in Bethnal Green because of the relatively static population and economic position of the area during the first half of the twentieth century. Much of this population was moved during the 1950s and '60s as the result of a planning decision to 'improve' the area and rehouse willing migrants in Greenleigh and elsewhere, and this was when many married children were separated from their parents who stayed behind in the old community (the proportion of older people surviving in a community is also obviously important in determining this type of relationship). Perhaps the type of geographical mobility which separates kin will increase as social mobility is fostered by the spread of education, although this factor itself could become relatively stabilized in time, as did the population and economic changes in places like Bethnal Green during the late nineteenth century.

There are one or two other historical sociological topics which may be briefly illuminated through the use of unfamiliar statistical sources. It is possible to calculate the age of marriage of different social groups as early as the eighteenth century.

[1] Macleod, op. cit.

Table 5

MEDIAN FIRST AGE AT MARRIAGE IN SUSSEX[1]

Period	*Labourers*		*All Occupations*		*Yeomen Farmers*	
(approx.)	*Grooms*	*Brides*	*Grooms*	*Brides*	*Grooms*	*Brides*
1757–69	25½	23	26½	23½	27	24
1788–1800	24	22	25	22½	25½	23

(each median was calculated from a sample of 100 cases)

In late eighteenth-century Sussex there was about one year's difference in the median age at first marriage between labourers and other occupational groups; this was true for both grooms and brides (although the difference is greater amongst grooms than brides). There is very little alternative evidence to check this finding; a brief analysis of the Nottinghamshire marriage licences yielded no significant difference in the age at marriage between different occupational groups. The age at first marriage differed between the two social classes defined for Maidstone: during 1837/38, of the 57 brides marrying grooms with unskilled occupations, 18 married below the age of 21, as compared to only 10 of the remaining 58 brides. Thus the Maidstone marriage statistics tend to confirm those for Sussex, although it does appear that the class differential in the age at marriage was widening throughout the nineteenth century: certainly the age at first marriage was rising amongst the aristocracy during the nineteenth century,[2] whilst among the total population it probably did not change much on average (this could mask changes between social classes, e.g. the age of marriage amongst the middle classes might have risen, whilst that among the working class fallen). Again further research is needed to settle this issue, particularly as it might have some bearing on the relationship between the age at marriage and the practice of birth control amongst the different social classes and how these factors changed over time.

Finally, there is one other subject which may be profitably studied through a neglected statistical source: attendance at communion service. The Anglican incumbents of Tenterden in Kent, noted the number of communicants during the main religious festivals for the period 1731–1848, although there are

[1] Ibid.

[2] T. H. Hollingsworth, *The Demography of the British Peerage* (supplement to Population Studies, Vol. XVIII, no. 2, pp. iv and 108, 205).

long gaps in the record.[1] I shall confine the discussion to the number of Easter communicants, as it reflects quite accurately the numbers of those at other times of the year, and the following table represents the predominant trends throughout the whole period.

Table 6

THE NUMBERS OF EASTER COMMUNICANTS IN TENTERDEN, 1731–1848

Date:	1731	1756	1761	1774	1781	1809	1848
No. of communicants	140	142	239	250	230	140	124

The number of communicants was more or less constant between 1731 and 1756, after which it rose very sharply. It is difficult to explain the rise between 1756 and 1761, as the number of baptisms fell slightly during the same period and only began to rise from 1763 onwards, which probably reflected an increase in population. The number of communicants reached a final peak in 1774, after which it began to fall slightly. Between 1781 and 1809 there is a complete blank in the record, and the figure for 1809 is markedly smaller than that for 1781. There were large fluctuations after this, although the final figure for 1848 was somewhat smaller than that for 1809. This decline in the number of communicants during the first half of the nineteenth century is all the more remarkable in the context of an expanding population: it increased from 2,370 in 1801 to 3,782 in 1851. The main decline in the number of communicants, however, appears to have occurred between 1780 and 1809, and although there are no population figures available for this period it is possible to express communicants as a proportion of baptisms. This proportion changed from just over 4:1 in 1731 to 5:1 in 1781, dropping sharply to under 2:1 by 1809. Making certain standard assumptions about the birth rate and the age structure of the population, we may estimate that about 40 per cent of the eligible population were communicants before 1780 and only about 10 per cent by 1848.

Some of the changes in the proportion of the eligible population who were communicants might be due to the policy of particular incumbents but this can hardly explain the long term trend. It is possible that some of the decline can be attributed to the emergence of Methodism during the relevant period. In 1790, Hastep estimated that there were in Tenterden, '2,000 inhabitants, of

[1] See the Tenterden parish register, lodged in the Kent County Record Office.

which about 500 were dissenters, who have two meeting houses here, one of Presbyterians, the other of Methodistical Baptists.[1] According to the religious census of 1851, there were in the Tenterden district (an area covering Tenterden and several surrounding villages) 7,412 total sittings, of which 2,650 belonged to dissenters. The increase in the proportion of dissenters—from about 25 per cent in 1790 to 35 per cent in 1851—cannot explain the degree of decline in the number of Anglican communicants in relation to the increase of population. There is no obvious explanation for this decline, and it might simply reflect the customary abandonment of ritual participation in Anglican services just in the town of Tenterden. Some contemporaries did note the religious apathy of agricultural labourers and Engels quoted the labourers who told a journalist in 1843 that they only went to Church because of it being a condition of receiving work and charitable concessions of fuel and potato plots.[2] It is possible that the creation of a landless agricultural proletariat through the enclosure movement may have destroyed the 'organic' sense of solidarity the poor are supposed to have felt with the rich before the enclosure movement, but this type of explanation involves an analysis of the changing social structure of Tenterden which it is not possible to pursue here. One specific factor might have been of some influence: the elimination of smallpox at the end of the eighteenth century. There was a general inoculation in the town in 1798 which appears to have covered all the vulnerable population; it is possible that the elimination of the great killer disease of smallpox removed one of the psychological reasons for religious worship (the early clerical opponents of inoculation predicted that it would have this effect): fear of death and disease.[3] Whatever the reasons for the decline of religious participation during the first half of the nineteenth century, it is clear that such a finding contradicts the conventional picture of this being a period of religious revival. Like the increase in Colyton pre-marital conception rate, the fall in the number of Tenterden communicants leads us to question historical generalization based purely upon literary evidence.

[1] E. Hasted, *The History of the County of Kent* (Canterbury, 1790), p. 98.
[2] F. Engels, *The Condition of the Working Class in England* (1958), pp. 303, 304.
[3] An example of the effect of disease on religious behaviour is the trebling of church and chapel communitants during and after the chlorea epidemic of 1849 in Merthy Tydfil. See *The Morning Chronicle* 15.4.50.

Practically all the statistical data in this paper has been about very specific localities, and England is a country with a history notorious for its regional variations. A great deal more research will have to be done before it is possible to make confident generalizations about any subject discussed in this paper, but as a great wealth of the relevant information is to be found in the Registrar-General's vaults and Public Record Office's ledgers, perhaps we can expect social historians and historical sociologists to do the sort of research required to reach definitive conclusions. No doubt many historical myths will wither in the process, possibly to be replaced by new ones in innocent statistical clothing. There are limits to the usefulness of statistics: how inadequate numbers are in describing the fact that six people died of starvation in Riseley, Bedfordshire, during the period 1690–1742,[1] but although these deaths are only casually recorded they do at least warn us against the myth of the pre-industrial golden age.

[1] See the Riseley Parish Register, *Bedfordshire Parish Registers*, XXVIII, in the Bedfordshire Record Office.

The *Morning Chronicle* survey is a unique attempt to describe all forms of working-class community life in England at a particular point of time. A large proportion of material has been selected from the original survey in order to provide information on all major aspects of social life of specific communities, and I hope in this introduction to illustrate how this part of our edited survey lends itself to sociological analysis. Not only will I attempt to illuminate the survey through the perspective of sociological ideas, but also in turn evaluate those ideas through the empirical case studies provided by the survey. The type of analysis that I shall adopt is only one of several to which the material might lend itself, and my aim is to merely illustrate the value of the survey to sociological and historical study. The starting point of my analysis is Lockwood's work on working-class images of society,[1] which takes the variations in the economic structure of community life as given and attempts to explain associated social and political attitudes as outcome variables.

It is possible to distinguish three types of worker: the traditional deferential, the traditional proletarian and the privatised. The deferential and proletarian workers both occur in their purest form in closed communities. The deferential worker typically lives in villages and small towns with a mixture of social classes, whereas the proletarian lives in one occupational communities like mining villages and working class town enclaves such as docking and shipbuilding neighbourhoods. The deferential works in a small workshop or in service and agricultural occupations where his relationship with his employer is personal and paternalistic; the proletarian worker is to be found in work situations which isolate him from his employer but unite him with his workmates. The deferential worker has a hierarchical “status” conception of social stratification whereas the proletarian has a conflict "us v. them" power model of social class. The privatised home-centred worker differs from both the deferential and proletarian workers in that he is not involved in local community life-this is a result of his residential mobility and lack of attachment to his workmates through the alienating quality of his factory work. This type of worker has a money model of social class and an "instrumental" attitude towards his work which is viewed primarily as a source of income; he forms the core of "the increasingly large section of the working class emerging from traditionalism".

Turning to the survey itself, deferential attitudes were found less among groups such as agricultural labourers but were found among certain classes of factory worker: Ashworth's country cotton factory of Egerton, Messrs. Arrowsmith & Slater's cotton factory on the outskirts of Bolton and Crawshay's ironworks at Merthyr-Tydfil.[2] Deference in all these cases manifested itself in respect and admiration for the paternalistic employer, and the absence of industrial conflict in the form of strikes. The key sociological factor in explaining the deference of these workers appears to have been the residence of the employer in the workers' community, combined with the paternalistic provision of model cottages, free medical provisions and the like.[3] It was possible for the employer to get to know his workers personally even in the context of a large factory work situation because of his prolonged residence in the community in which many of his workers had spent their whole working lives. This of course was not typical of factories in large towns such as Manchester where frequently workers had never even spoken to their employers; for example the Manchester cotton worker who stated that "I have worked in that mill sir, these nineteen years, and the master never spoke to me once".[4] But even in Manchester some of the factory owners were involved in a more personal capacity with their workers through the Sunday School movement, which appears to have helped spread their values of self-help and individual achievement and discouraged radical political activity such as Chartism.[5]

The deference of factory workers is of greater historical importance than most sociologists or even historians have recognised. During Luddite attacks on cotton-factories during the beginning of the nineteenth century it was often the factory workers who helped their employers defend their property

and there are examples of factory workers actually firing on and killing Luddite attackers.[6] Many of these early factories were in rural areas where the employers lived in an almost gentry-like relationship with their workers, generating a classical deference situation. Perhaps as important as this social situation though, was the fact that these early factory workers were very highly paid in comparison to their contemporaries such as hand-loom weavers. As well as high income, these factory workers benefited from the sort of paternalistic provision of model cottages etc. discussed above. The influence of paternalistic intervention on behalf of workers is illustrated by the case of the London coal-whippers: the quality of their lives had been dramatically improved through the intervention of the government in legislating against payment of wages in public-houses. They and their wives appear to have been highly appreciative of the government's action, which would go some way in explaining why they "were extremely proud of their having turned out to a man on April 10, 1848, and become special constables for the maintenance of 'law and order' on the day of the great Chartist 'demonstration' ".[7]

The relative absence of deferential attitudes amongst agricultural labourers in the survey can be explained in terms of the fact that they benefited so little from their "personal" relationship with farmers and gentry.[8] The poverty and destitution of the agricultural labourers is more than adequately described throughout the survey, but in order to indicate the unfeeling way they were sometimes treated by farmers I quote the following casual description of their treatment in an area of Norfolk from an unpublished letter:

> It is the usual custom, I was informed in a great many of the adjoining parishes, for the farmers to send their teams in order to convey their labourers with their families to the union workhouse for the winter months, and as many as seventy persons have been seen thus to pass through Trimmingham on their way to the union workhouse.[9]

It is not surprising that rick-burning was a frequent occurrence in East Anglia (where large-scale capitalist farming was most frequently to be found), and in this respect agricultural labourers were solidly proletarian in their attitudes.[10] This reference to rick-burning should however remind the careful reader of Hobsbawm and Rude's book on the riots of the agricultural labourer in 1830 that in many cases landlords managed to recruit "respectable labourers", "servants and retainers, grooms, huntsmen, game-keepers" as special constables to put down the riot.[11] There is some evidence to show that domestic servants were the most deferential occupational group during this period,[12] and it is unfortunate that the survey was terminated before they were studied (they were about the largest single occupational group in the country). The "respectable labourers" referred to above were presumably labourers who worked on the estates of paternalistic landowners and lived in the "closed" villages attached to these estates. These labourers benefited substantially in economic terms from their relationship with their employers,[13] as did domestic servants particularly those of the aristocratic rich.

Deference arose not only out of a personal relationship with a paternalistic employer but also with other elites such as the clergy. This is illustrated by a description of the religious behaviour of agricultural labourers:

> In small parishes, where the clergyman is frequently brought into personal contact with the labourers, and where, from other causes, he exercises a direct influence over them, they may be found pretty regular in their attendance at church; but generally speaking their attendance is neither large nor constant, most of them moping about on the Sunday, smoking and drinking, and some of them spend nearly the whole day in bed.[14]

This suggests that without the influence of the clergy, labourers had very similar attitudes towards the Church as did their urban proletarian contemporaries. The term influence is rather ambiguous with

respect to the analysis of deference, for although there is some evidence that labourers did on occasions genuinely identify with the hierarchical values of the Church, there is even more evidence that many of them attended church in a rather mechanical way as a result of economic pressure.[15] The clergy were often in a position to exercise control because of the desperate economic plight of their parishioners. A labourer's wife living in Cambridgeshire told the rural correspondent in October 1850:

> I've just put the last twig of wood on the fire. I went to the clergyman this morning, and I asked him for God's sake, to give me sixpence to buy a bit of firing with; and he said he could not afford it, and that he was as bad off as I was.[16]

When the clergy were able and willing to use their wealth, they were capable of increasing their congregation significantly. An example of this is when the new vicar of Sutton Courtenay discovered the neglect of church attendance by the labourers living in his parish, partly through the use of his wealth he "got the people into the habit of attending church, which he deemed to be his first duty".[17]

Several communities described in the survey show some sociological features typical of the proletarian worker: the London and Liverpool shipwrights, the Middleton weavers, the Cornish fishermen and miners, the Swanage stone quarriers, and the London coopers and hatters.[18] However, the fullest description of the proletarian worker in purest form is the account of the Durham and Northumberland miners.[19] The sharing of egalitarian values in these village mining communities took a religious form during this period and most of these miners were members of Methodist congregations. The class basis of Methodism and the linked antagonism towards the Church of England was explicitly recognised, as the manufacturing correspondent revealed:

> The Church of England is, I believe, from what I have seen, regarded by a large proportion of the mining community with feelings of positive and active enmity. They almost invariably class it with the aristocratic institutions and influences which they believe to be hostile to them. The church clergymen, they say take the part of the masters, but the Ranters (Primitive Methodists) take part with the men, addressing their comrades in their own patois, and treating every scriptural subject in the peculiarly technical tone which is common to the whole community.[20]

This class basis of Methodism was expressed during the great strike of 1844:

> A religious feeling came to be strangely mixed up with the movement. The Ranters' chapels were crowded, and the success of the strike was prayed for from the pulpit. The people went to chapel and prayer meetings, as they said, to "get their faith strengthened".[21]

This description of the use of religion as a way of expressing and reinforcing community solidarity is a classic example of what Durkheim conceived as the main function of religious activity, although the religious "rationalisation" of class interests is more akin to a Marxian analysis.

Unlike the Durham and Northumberland miners, those living in Staffordshire showed little class solidarity amongst themselves. One major reason given for this difference was given by a Staffordshire miner when commenting on the fact that miners in the North were much more active in organising a trade union than those in Staffordshire itself: "They may get it (the union) up again ... in the North, but we're people from a great many counties here, and we don't trust each other."[22] This is a good illustration of the importance of geographical mobility in determining class solidarity. Two other major reasons for the lack of unity amongst the Staffordshire miners were: (i) they did not live in small separate villages as did the northern miners but inhabited undifferentiated areas of sprawling township shared with iron-workers and others; (ii) they worked in relatively small mines run through middle-

men with whom they had some kind of personal relationship, again unlike the north where the mines were run on much more bureaucratic lines. One of the effects of the lack of community solidarity amongst the Staffordshire miners was the absence of any religious consensus such as an attachment to Methodism, and their style of life was characterised by a pub-centred culture and a liking for rough traditional sports.[23]

One of the difficulties in giving a clear-cut theoretical analysis of a survey like that made by the *Morning Chronicle* is its richness of empirical detail. However, this sometimes allows us to extend the analysis by directly using comments reported in the survey; for example, the London turner who noted because of the noisy nature of his work "no talk can be carried on, as in a tailor's shop, by which men can pick up a little politics or knowledge".[24] This adds to our understanding about the range of variables to be considered when attempting to account for social facts such as proletarian class consciousness. The noisiness of their work is also stated to be one of the reasons why the "honourable" cabinetmakers were so little interested in politics and might also help to explain their relative indifference to trade unionism. Other reasons only implicit in the survey were the absence of any noted concentration of the trade in any particular area of London and perhaps most importantly the fact that most of these London cabinet-makers were "countrymen" in origin.[25] By contrast the London shipbuilders were "mainly natives of the metropolis" who lived "chiefly in Poplar and the adjacent parts"- and presumably these geographical foundations of a closed community life go some way in explaining the fact that "not a few of the shipbuilders have brought up their sons to their own calling". All the three above factors can be seen as contributing to the political awareness of the shipbuilders described as follows:

> The shipbuilders are, I found, great politicians. It is customary during their half hour's luncheon at eleven o'clock, for one man to read the newspaper aloud in the public-house parlour; a discussion almost invariably follows and is often enough resumed in the evening.[26]

It is not possible to tell from the survey which factor is the most important in determining such political consciousness, a question which could only be settled by a sophisticated methodology of statistical comparison directed at an explicitly formulated theoretical proposition. Although the survey has none of this, at least it produces a richness of description which forces us to recognise the complexity of situations to be explained, and the inadequacy of generalisations to account for all the detailed variations. It is for this reason that there is no effective summary substitute for the survey itself.

The privatised worker is more difficult to find in the survey than the deferential and proletarian types and this is what would be expected from the formulation of the privatised worker as belonging essentially to the twentieth century. There are however indications that there was a nineteenth-century equivalent to the privatised worker, particularly where there was an absence of deferential or proletarian community life. For example, the London cabinet makers were not only relatively indifferent to politics and trade unionism but were described by Mayhew in the following terms:

> The great majority of the cabinetmakers are married men and were described to me by the best-informed parties as generally domestic men, living, whenever it was possible, near their workshops, and going home to every meal. They are not much of playgoers, a Christmas pantomime or any holiday spectacle being exceptions, especially where there is a family. "I don't know a card-player", said a man who had every means of knowing, "amongst us. I think you'll find more cabinetmakers than any other trade members of mechanic's institutes and literary institutions, and attenders at lectures."[27]

This was the labouring aristocracy of "respectable" artisans who not only prided themselves on their education and their "rational" amusements, but also on the fact that they could maintain. their family without their wives having to work; for example, according to a bedstead-maker

> Several of us are house-keepers and can support our wives and families comfortably. I don't think one of the wives of the members of our society work in any way but for the family.[28]

One of the results of this type of family-centred respectability was that the homes of these artisans were very well furnished; for example, in the homes of the "honourable" cabinetmakers

> you have the warm red glow of polished mahogany furniture; a clean carpet covers the floor; a: few engravings in neat frames hang against the papered wall; and bookshelves or a bookcase have their appropriate furniture. Very white and bright-coloured pot ornaments, with sometimes a few roses in a small vase, are reflected in the mirror over the mantleshelf.[29]

Given that the privatised worker is not only family-centred but also home centred (the two are obviously intimately connected), we can take this evidence about the well-furnished homes of the "respectable" artisan as a further indication of their relatively privatised state.

The association between a comfortable home and the emotional importance of the family is hinted at by the manufacturing correspondent who implied that "respectability " was not confined to particular occupational groups:

> Before leaving the subject of house and street architecture I may be permitted to observe on the constant recurrence of a phenomenon which I have remarked on in many industrial districts in England. In the houses of the worst class - in those the inhabitants are slatternly and poor- the seldom failing pictorial decoration upon the walls is derived, with significant frequency, from the illustrations of some highwayman novel. In more comfortable dwellings although occupied, perhaps by individuals of the same nominal rank in the social scale, you may find a stiff family portrait or two-probably a crown or half-a-crown's worth from some vagrant artist; or, perchance, there are engravings of some Chartist or Radical leader belonging to the political school of the pater familias.[30]

The two categories of person described in this passage correspond more or less to the distinction familiar to sociologists between the "roughs" and the "respectables". One of the chief differences between these two groups lay in the focus of their leisure activities: the "roughs" spent most of their spare time in the pub, the "respectables" at home with their family. The survey provides much more evidence about the first than the second, although the constant references to the sobriety of the respectable artisan is indirect confirmation of the latter. An example of this distinction within virtually the same trade is to be found amongst the tanners and the curriers; the London tanners were traditional "roughs" with a pub-centred and prize-fighting sub-culture, as contrasted with the educated home-centred "respectable" curriers. Part of the explanation for this difference lay in the fact that the former all worked in the area of Bermondsey and thus formed a traditional community, whereas the latter worked and lived in different parts of London leading to a degree of privatisation.[31]

There is one problem with using the term privatisation with reference to the mid-nineteenth century period and that is that it assumes a sharp distinction between the work and home situations. Domestic workers by definition worked in the home and where they worked long hours they were effectively privatised; for example the London fancy cabinet-makers were

> far less political than they used to be. The working singly, and in their own rooms, as is nearly universal with them now, has rendered them more unsocial than they were, and less disposed for the interchange of good offices with their fellow workmen, as well as less regardful of their position and their rights as skilled labourers.[32]

Lockwood however has used the term privatisation to refer to non-work activity, but with these domestic workers leisure-work activity hardly existed. This was not always the case for the small master turners living and working in Spitalfields were

> rare fellows for skittles, cards, and dominoes, and badly as they're off, numbers of them don't work on a Monday.[33]

There are also cases in the survey of workers not working at home but working such long hours that they in effect had no leisure time at all, and perhaps an extreme example of this is the London omnibus drivers and conductors who did not have the time to attend church on a Sunday.[34] Although it is technically correct to apply the term privatisation only to non-work activity, the social and political effects of both work and leisure situations are considered in the analysis, and it is only necessary to add amount of leisure time as a variable in order to extend it to nineteenth-century conditions.

One theme which constantly recurs in the survey is that of wives who had spent their childhood and adolescence as factory workers being less capable of looking after their families than women who had been domestic servants before marriage. One of the results of the lack of domestic competence of ex-factory wives was to discourage husbands from spending their leisure time at home and lead them to spend more time in the local pub. Perhaps even more important than this in creating a pub-centred culture was the lack of elementary domestic comforts such as adequate lighting, heating, furniture, space and cleanliness - all particularly important to make a home an attractive place during a period when fertility and the number of children living was so high. The effect of the lack of lighting and heating in creating a pub-centred culture is beautifully illustrated in a letter on the Liverpool docks where ships were forbidden to have either heat or lighting because of the fear of fire.[35] In this respect the privatisation of the worker can be seen as a function of improvements in the quality of the home, and this was tentatively suggested by the rural correspondent in a discussion of the labourer's home.[36] In fact the home situation is an equally important sociological variable as the work situation, and not only with respect to the comfort of the home but also home situated technology. For example, the introduction of running water instead of water from wells increased women's home centredness and eliminated the social relationships based on the communal collection of water.[37]

Although the survey can be interpreted in very general terms as confirming the analysis of different types of working class, the characteristics of nineteenth- century "respectables" do not fundamentally fit into the appropriate classification. This class was not only relatively privatised but also much more aware of itself as a social class with distinct radical political views than the "roughs" who, in spite of their shared community style of life, were not as aware of being a part of the "working class".[38] One of the major reasons for this difference was the greater literacy and education of the "respectables"- and in the nineteenth century working class education was intimately linked with "self-improvement", "temperance" and relative political awareness. In sociological terms "self-improvement" and "temperance" can be translated into privatisation, whereas political awareness can be seen as growing out of the rationality of an educated class. Even the evidence which most appears to support Lockwood's analysis presents contradictions and ambiguities; for example, the mining communities of Durham and Northumberland were in some sense both traditional proletarian and privatised. A lot of the leisure time of these miners was spent at home working on and around the house and even their pub-culture had become a residue of its past importance. One of the reasons for their home centredness

was that there was no employment available for women in the area and as a result wives were "the great agents in getting the houses as well furnished as they are".[39] Miners also had a relatively short working week which enabled them to spend leisure time at home and they could afford to furnish their houses comfortably because of their relative affluence compared to other working class groups. One probable effect of their home centredness was their Methodism, for it was a religion which emphasised and expressed what for the sake of brevity one might call the domestic virtues: cleanliness, temperance, and family respectability.[40]

With reference to the contradictions and ambiguities in the classification of different types of worker, it could be argued that the categories are "ideal-types" and that we should not therefore be concerned with empirical exceptions. This type of argument which is popular amongst some sociologists would mean that analytical power of any classification could never be evaluated, nor an analysis refined so as to more adequately explain empirical evidence. An example of how Lockwood's analysis would have to be modified to explain an awkward fact is provided in the survey's account of the relationship between workers and their employers in Oldham.[41] This town was noted for its large number of small employers who rented "floors or small portions of factories dirty and constructed in the old-fashioned unventilated style".

One informant stated that

> these masters ... are just the same as if they were the fellow-workmen they employ. They dress much in the same way, they live much in the same way, their habits and language are almost identical, and when they 'go on a spree' they go and drink and sing in low taverns with their own working hands.

These "operative employers" had themselves been ordinary workers before becoming employers, which explains their similar styles of life. On Lockwood's analysis the close relationship between workers and employers would be expected to lead to a sense of community between them, inasmuch as he implicitly assumes a one-to-one relationship between social interaction and social solidarity. In fact the survey informants could not agree on whether the social relationship between the Oldham employers and their workers was more harmonious than that between large capitalists and their workers. The position was summarised by one informant when he stated that "although masters and men often caroused together yet, on occasions of difference arising between them, the masters would get terribly abusive and terribly bad blood would ensue". In extreme cases this "bad blood" led to a complete transformation of the social relationship between employer and employee; for example in Banbury "the workers of one small firm applied to join the union only when this· personal relationship had broken down: the boss said to be worse for drink had abused and sworn at them".[42] This type of situation suggests an emotionally ambivalent attitude as characterising the relationship between employer and worker, leading to a mixed deferential/proletarian type of response. The emotional ambivalence is presumably the result of a tension between friendly personal interaction on the one hand and the latent economic conflict between employer and worker on the other.

The use of the term emotional ambivalence is a pointer to the way in which the descriptive analysis employed in this paper can be developed so as to make the assumptions employed theoretically explicit. Implied in this analysis is the assumption that the common theoretical ingredient to all the sociological classifications is a psychological one. This is a position which has most recently been associated with Homans's work, which has increasingly emphasised the importance of rewarded behaviour in determining the effects of social interaction.[43] In this paper the two main "rewards" of social interaction considered have been economic benefits and friendly personal interaction. Although Homans's analysis would go a long way in explaining the social processes discussed under the various

classifications of different types of worker, there are a number of key problems which fall outside of the scope of this type of work. One such problem has already been briefly touched upon in the discussion of labourers' attitudes towards the Church. Although they rarely appeared to have identified with the values of the church, they frequently attended it as the result of economic and social pressure of the local gentry etc. This type of behaviour may be seen as socially "defensive" against the power of controlling elites, and it is possible to find many examples of domestic servants and agricultural labourers presenting a deferential front to their employers, and then giving vent to their true feelings of hostility when amongst themselves.[44] It is therefore necessary to make the distinction between social behaviour and the internalisation of values in order to explain some of these ambiguous and ambivalent situations. The only psychology to attempt an explanation of the internalisation of values in any convincing manner is psychoanalysis,[45] and I will try to develop the theoretical assumptions of the typology of different types of worker by examining the category of deferential worker in the light of psychoanalysis. I will then illustrate how this theoretical development can be applied to the survey in such a way as to further illuminate it, although with the explicit recognition that any sociological generalisation does inevitable violence to the complexity of empirical reality and to that extent must always remain at the level of "ideal type" analysis.

The attitude of the employer towards the deferential employee has invariably been described as "paternalistic"; an example of this is to be found in Sturt's Wheelwright's Shop where he describes how "the men sought his (father's) advice as if they were his trusting children".[46] Psychoanalysis interprets deference as the employee "transferring" his admiration and acceptance of parental authority onto his employer. As well as transference, it is possible to detect the psychological process of identification in the deferential social relationship, with the employee identifying with his employer's social value and imitating his style of life. Transference and identification are both grounded on the long period of dependency during childhood socialisation, although identification occurs through the child gradually succeeding to a position of equality with its parents (identification can also occur with brothers and sisters-this form of identification plays an important part in the formation of proletarian egalitarian values), whereas transference represents the unconscious residue of childhood dependency. Where a deferential social situation takes the form of a hierarchy with limited social mobility, such as that among domestic servants, deference is likely to occur as described by Lockwood and be primarily a question of transference. In this type of situation identification is mostly vicarious, although the literature suggests that the servant imitates aspects of his employer's style of life as well as identifying with him at the level of phantasy.[47] The hierarchical nature of the servant household allows the upper servants to imitate certain features of their employer's authoritative behaviour, which fosters identification through a narrowing of power and status differences. The model that the deferential upper servants have of the hierarchy (and social stratification in general) is likely to be a three status-group one, with himself in the middle and socially superior to the lower servants and the "lower" classes. Lockwood has also mentioned the small workshop as an example of a deferential social situation, and where there is a minimum of social mobility this is likely to be the case.[48] During the nineteenth century however, the small workshop was still associated with the traditional pattern of apprenticeship arid journeyman status (to some extent) being preparations for succession to independent master status. Such a social situation fosters identification rather than transference, a point which will be returned to at the end of this introduction.

Identification and transference constitute the key psychological ingredients for distinguishing genuine deferential attitudes from deferential behaviour presented as a defence against the power of the employer. The key sociological factor in producing genuine deferential attitudes (via transference and identification) is the presence of an enduringly rewarding relationship with the employer, particularly with reference to economic benefits and friendly interaction.[49] This rewarding relationship may be seen as producing transference and identification through the reactivation of the experience of being

loved as a child by a parent, although psychoanalysis has developed a number of highly complex ideas on this subject which go far beyond this rather simple formulation of the problem (for example Anna Freud's notion of "identification with the aggressor"). It would be possible to extend the present analysis in a number of ways in order to more fully account for the complexities of the social relationships under discussion. Deference can be seen as a "reaction- formation" designed to disguise the unconscious hatred that the powerless feel towards those who exploit and wield power over them - a disguise that the exploited accept through the repression forced on them by their social situation of powerlessness (the classical example of this is of course the "Uncle Tom" syndrome amongst negroes in the American South). The complicating of the analysis in this way leads us away however from the *Morning Chronicle* survey which only provides information sufficient for a simplified form of analysis.

The value of extending the sociological classification of types of worker in a psychological direction may be illustrated by an analysis of religious belief and affiliation. The survey provides us with a starting point through its description of the religious life of various communities. I have already touched on the Methodism of the Northumberland and Durham miners; the survey also suggests that the Cornish miners were also almost exclusively attached to Methodism. Other than the miners, the community which showed the greatest attachment to Methodism were the Cornish fishermen. The common features of these occupational communities were the relative autonomy of the men in their work situations, freedom from a hierarchical relationship with employers, the isolation of the community from other occupational groups and the absence of feminine employment and the resulting home-centredness discussed above. Of these factors, perhaps autonomy and freedom from hierarchical control were the most important in determining religious non-conformity with its emphasis on the ultimate moral responsibility of the individual and the rejection of religious hierarchy.[50] The Cornish fishermen were typical of the social strata of small tradesmen who were the social backbone of religious non-conformity, and they shared in the additional personal freedom associated with the ownership of capital and self-employment. The Cornish miners also shared in the profits of the mine, although they owned little or no capital and were partly subject to the authority of the "captain" of the mine who represented the economic interests of the mine owners. Many of these "captains" had previously themselves been ordinary miners and rose not only to positions of authority within the mine but also in local Methodist chapels where they were frequently lay preachers.[51]

This example of the "captain" lay preachers points to the importance of the psychological mechanism of identification (in this case with the authority of the employer) in spreading the Liberal religious non-conformity of the manufacturing middle class. We have already seen how Manchester manufactured and fostered this identification through the Sunday School movement, and this was particularly effective in the cotton industry because of the very large numbers of owners and managers recruited from the operative ranks.[52] The spread of Liberal self-help values through the identification of workers with their employers probably occurred most frequently in the traditional workshop situation. Small workshop situations have not therefore always been associated with hierarchical Conservatism, but also (and perhaps more frequently) with Liberal individualism. The psychological process of identification was more important in the spread of the latter, transference in the diffusion of the former. With the growth of large-scale industry however, many non-conformist manufacturers acquired the same characteristics as their Anglican counter-parts, until by the twentieth century it was difficult to distinguish them in terms of their behaviour towards their employers.[53]

NOTES

1. D. Lockwood, “Working Class Images of Society”, *Sociological Review*, 14 (1966).
2. Where letters have been included in our edited survey, I shall simply state the relevant district and letter number. In the present instance, these are Manufacturing IV (Manufacturing District, Letter IV) and Merthyr Tydfil VI (Merthyr Tydfil District, Letter VI).
3. For other historical examples of deferential factory workers, see J. D. Marshall, “Colonisation as a Factor in the Planting of Towns in North-West England”, in Dyos, op. cit., pp. 226-8; C. Townson, *The History of Farington* (1893), pp. 23, 24, 31, 33.
4. Manufacturing III.
5. Manufacturing IX.
6. The most famous of these incidents is the attack on Cartwright's Rawfolds mill, which was defended by a combination of workers and soldiers. See J. L. and B. Hammond, *The Skilled Labourer* (1919), p. 305. More interesting from a sociological point of view is the attack by colliers and weavers on Burton's Middleton factory in 1812, which was exclusively defended by Burton and his workers. The killing of a number of the attackers led to retaliation the next day when Burton's house and his workers' cottages were attacked. See S. Bamford, *The Autobiography*: Volume 1 (1967), pp. 300-5, and the Hammonds, op. cit., p. 289. No scholarly study has ever been made of the defence of factories subject to Luddite attack, but for evidence on the role of factory workers in their defence, see the Hammonds, op. cit., pp. 195, 285.
7. Metropolitan XIX and XXIV.
8. Physical proximity did not guarantee a "personal" relationship. See Rural XV about East Anglian labourers who claimed that it was rare for the farmers "to condescend to speak to them, except in terms of reproach or abuse."
9. Unpublished Rural XIX.
10. See Rural XV for a description of rick-burnings and the bitterness felt by many East Anglian labourers towards their farmer employers.
11. E. J. Hobsbawm and G. Rudé, *Captain Swing* (1969), pp. 131, 155, 156.
12. For an almost pure case of deference of someone in the position of a servant, see H. Mayhew, *London Labour and the London Poor*, Vol. 2 (Dover Publications, 1968), pp. 467-71.
13. For a description of such "respectable labourers" and the model village in which they lived, see F. Thompson, *From Larkrise to Candleford* (1948), pp. 274-87.
14. Rural XIII.
15. These economic controls did not only involve the administration of charity by the clergy but also the use of threats about employment and tied cottages coming from farmers and landlords, forcing their labourers to church. For a vivid example of the use of a range of economic controls for this purpose, see F. Engels, *The Condition of the Working Class in England* (1958), p. 304. For a description of labourers who appear to have genuinely identified with the values of the church, see S. Mays, *Reuben's Corner* (1969), pp. 78-88.
16. Unpublished Rural XXXVIII.
17. Unpublished Rural IV.
18. Metropolitan LXVIII; Liverpool XVII; Manufacturing XII; Rural X, XI & XIII; Metropolitan LXIX & LXXVII. The London dockers did not form a cohesive community despite living in a specific enclave in the East End. The main reason for this was the casual nature of the work and the rapid turnover of the labour force (see Metropolitan III & IV). The costermongers of London, on the other hand, were a highly cohesive class with their own very distinctive sub-culture, despite living in different parts of London. The explanation of their cohesion lay in the hereditary transmission of the occupation from father to son and the mobile nature of their work, leading to constant contact with each other. For a brilliant description of this sub-culture, see H. Mayhew, op. cit., Vol. 1 (Frank Cass, London, 1967), pp. 4-104.

19. Manufacturing XVIII to XXII.
20. Manufacturing XXI.
21. Manufacturing XVIII. There is some evidence, however, that Wesleyan Methodist ministers opposed the strike, and that it was the Primitive Methodists that the survey was referring to. See R. F. Wearmouth, *Methodism and the Working-Class Movements of England 1800-1850* (1948), p. 189.
22. Manufacturing XXIII.
23. It should be pointed out, however, that there was a residue of this traditional culture among the Durham and Northumberland miners as well.
24. Metropolitan LXVII.
25. Metropolitan LXIII.
26. Metropolitan LXVIII.
27. Metropolitan LXIII.
28. Metropolitan LXIII.
29. Metropolitan LXIII.
30. Manufacturing XXX.
31. Metropolitan LXXVIII.
32. Metropolitan LXIV.
33. Metropolitan LXVII.
34. Metropolitan LXXI.
35. Liverpool III.
36. Rural XXXIX.
37. For examples of such communal relationships, see Birmingham II and Merthyr Tydfil I. In our own day, television is by far the most important form of home-situated technology in bringing about privatisation.
38. See the evidence already quoted in this introduction; for the differences between "respectable" artisans and "rough" unskilled labourers, see Metropolitan XIX. The "working class" discussed by E. P. Thompson in his *The Making of the English Working Class* is mainly made up of skilled artisans and weavers, who, in the context of the present discussion, were relatively privatised.
39. Manufacturing XX.
40. For example, the "sober habits" of the Bilston Methodists led to domestic cleanliness and a relative immunity to the ravages of cholera. Manufacturing XXIV.
41. Manufacturing VIII. It should be pointed out that the "account of social relationships and the nature of industry in Oldham is at complete variance with that presented by Foster in his study "Nineteenth Century Towns: a Class Dimension", in H. J. Dyos (ed.), *The Study of Urban History* (1968).
42. M. Stacey, (1960), p. 28.
43. See G. C. Homans, *Social Behaviour: Its Elementary Forms* (1961).
44. See Powell, op. cit., pp. 79-81 for an example of this amongst domestic servants who at times came to expressing amongst themselves proletarian attitudes of "us" opposed to "them." A number of similar examples amongst agricultural labourers can be found in F. Thompson, op. cit., but see especially pp. 50, 51. The "defensiveness" of domestic servants is illustrated in Rural XIII, which gives a description of the secret language used by servants to hide from their employers' meetings with the opposite sex.
45. For an attempt to assess Weber's typology of internalised authority in psychoanalytical terms, see Donald McIntosh, "Weber and Freud: on the nature and sources of authority", *American Sociological Review*, 35 (October 1970).
46. George Sturt, (1963), p. 55.
47. See, for example, Margaret Powell, *Below Stairs* (1968), pp. 77-79.

48. Sturt, op. cit., pp. 12, 55, 113, 201.
49. Powell, op. cit., pp. 129, 130.
50. One of the main reasons for the Nonconformist Liberalism of town tradesmen was their freedom from the hierarchical control exercised by the Anglican Tory gentry. See J. R. Vincent, (1967), pp. 15-18. This can be related to the tradition of the "free-born" Englishman, which found its most politically effective form in the Leveller movement - and the Levellers rejected the inclusion of servants in the franchise "because they depend upon the will of other men and should be afraid to displease them." See C. B. Macpherson, *The Political Theory of Possessive Individualism* (1962), p. 123.
51. Rural XI.
52. S. J. Chapman and F. J. Marquis, “The Recruiting of the Employing Classes from the Ranks of the Wage Earners in the Cotton Industry”, *Journal of the Royal Statistical Society,* 75 (1912), 293-306.
53. See the *Sunday Times*, May 10, 1970, pp. 3, and May 17, 1970, pp. 54, 55, for the paternalism of the Nonconformist Clark and Pilkington families.

THE POOLS WINNERS

INTRODUCTION

PART II

Introduction (2)

The three chapters in the following section are all based on a survey of pools winners carried out mainly in the summer of 1972. Using press reports, we selected people who had won £75,000 or more at 1957 money values, the equivalent of about £160,000 today. The figure of £75,000 was chosen for two reasons: first, it was sufficiently large to make a marked difference in economic circumstances; second, it was the most that anyone was allowed to win between 1951 and 1957, when the pools companies were operating their voluntary limit. Nobody who had won after December 1970 was included in the study: this was to allow time for them to respond to their win before being interviewed. In all, we traced 191 winners who met our criteria: the earliest win was made in 1937, the latest in the autumn of 1971. Fourteen of these had died before the research had started, and subtracting the thirty-two who remained anonymous and the one we were unable to trace, left 144 winners who were still alive and traceable. Six of these were living abroad. The remaining 138 were all contacted and approached for an interview. Eighty-nine agreed and completed a specially prepared questionnaire schedule; forty-nine refused to participate in a formal interview (although over a third were prepared to talk to us at some length on an informal basis).

Some of the forty-nine who refused may have done so because they were reluctant to revive unpleasant memories; it is therefore possible that the following chapters under-estimate the problems that winners experienced. The evidence that we have, however, suggests that most declined because they did not want to risk further publicity: thirty-eight per cent explicitly mentioned this as a reason. There are also technical factors about the way the samples were

compiled which exaggerated the number of non-respondents. We obtained further information from the *respondents* on the size of their win and who they shared with, which allowed us to exclude about half a dozen for not winning enough. This was not possible with those who refused to be interviewed; presumably an unknown number should also have been excluded.

It is important in a study of this kind that there be few basic differences between those who participated in the survey and those who did not. Fortunately, we have a great deal of information on the two groups, from initial discussions about the interview, informal conversations, press reports and data collected on all pools winners in the process of tracing them (from electoral registers, rating lists, etc). As far as we can tell from all this information, the two groups do not appear to be radically different. Both live in similar types of houses (paying almost identical average rates), own the same sort of cars and wear similar clothes; all of which suggests that they live in the same kind of economic circumstances. Both came from similar occupational backgrounds, were of identical average age at the time of their win, and moved within the same radius from their original home. Our subjective impressions of the way they had adapted, also suggested that there was little difference between the two groups.

To provide an additional means of assessing the effects of the win, a sample of non-winners from the general population was selected to form a basis of comparison. Each one of the eighty-nine winners who completed our questionnaire was matched with someone from the general population, of the same age, sex and social background (in the case of the pools winners, before their win). The people in the comparison group had also done the pools at some time during their lives. Both the pools and the comparison group answered the same set of questions in the first half of our questionnaire: on property and possessions owned, health, attitudes, occupations, etc. The answers to these questions are discussed, along with other evidence from the research, in the following chapters.

CHAPTER IX

Winning And Spending

How do you win the pools? Approximately fourteen million people try each week, even though the odds against any one person having a really big win, like £130,000, are about eighty million to one. The pools companies claim that winning involves an element of skill, that it is possible to predict the results of matches from a knowledge of football and the form of the teams. Indeed, under the present betting laws, the very legality of football pools depends upon this argument, since if results were simply a matter of chance, they would be, technically, a lottery and as such, against the law. The winners too are sometimes made aware of the pools companies attitude: "The teams were picked out of a water jug but the pools company don't like us to say that, they like a bit of skill in it." How far then is skill involved in the big wins, and is it possible to learn how to win from the methods of the very successful members of our sample? One way to answer this question is to see how many thought they had used skill in making their selections.

The problem that they all had to face was, of course, that if they simply followed form and possibly predicted the correct results, then so would many of the other millions doing the pools that week, and the amounts paid out would therefore have been very small. To win the maximum prizes, the £750,000 wins, not only do you have to get the results right, but you have to do so when no-one else does. One winner, for example, described how he made his selections, and made little pretence of skill:

"I had about twelve teams I had a notion of . . . When you look up the columns you see some team has maybe gone ten weeks without a draw: well I would say to myself, that one's

entitled to their turn. That's the only system I had and who can predict what twenty-two men are going to do?"
However, the week he won, he guessed that a previously very unsuccessful team might draw with Glasgow Rangers: "That would be like some fourth division team playing Tottenham or Chelsea, I was favouring them at that time because they were trying hard, it was their first go in the first division."

Others too, in our sample used this "anti-form" approach predicting the results not so much by choosing teams which should win on their past record, but by selecting them because they hadn't had a draw for some time. One of them explained:

"I'm not a heavy gambler, like, but it don't matter what gambling I do, I use the law of averages, if there is such a thing. I sort of use the previous three weeks results and, say if there were, for instance, two or three draws coming up on the same numbers, I keep away from them and do something that hadn't come up as a draw before – and it did work out that way actually."

It is the random nature of the pools that enables so many "chance" coupons to result in large wins and is the method that women in particular are likely to use. The pools companies provide simple random systems for "ladies" to cater for this market and therefore enable those with little or no interest in football and certainly no claim to skill to enter. For example, one woman in our sample who won well over a third of a million pounds said, "I don't understand the pools, I haven't the faintest idea. It was an easy one a "ladies" coupon and all I had to do was put ten crosses, I didn't have to do this 'accummulating' or whatever it is they have to do, the men. That was all I done. I just put ten crosses down, filled me name and address in, and gave the man 22½p when he came on Thursday. And that was it and I'm still doing it now!"

The weekly coupon can become quite an obsessive ritual in which the same sets of numbers are used for years. In one such case, the winner who won more than ten years ago made a mistake in filling in his usual entry and wanted at the last moment to fetch another coupon from his collector to make

sure his customary entry was sent in. It was only with great difficulty that his wife managed to persuade him to leave it and post it as it was. Their win today would be worth approximately £240,000. Among the sample as a whole, however, fifty-five per cent used systems based on chance – birthdays, numbers picked at random from a tin or with a pin – as the method of filling in their successful coupon; sixteen per cent made their selections on the basis of form and nine per cent used a combination of both methods. Of those remaining it was not possible to be certain from the descriptions winners gave of their systems whether skill or chance was used (if there was any doubt they were excluded; therefore the percentages probably underestimate the proportion employing chance methods for completing their coupons). Thus over half the sample admitted that they did not use skill in filling in their winning coupons and it would seem that there is little point, therefore, in attempting any other method, particularly as none of the sample had managed to pull off a first dividend twice, however "skilful" they thought they might be. However, one winner who we interviewed won over £87,000 in one week in the 1940s, betting with a complicated statistical system, "investing" thousands of pounds on each coupon. He was in fact the only one we interviewed who could be described as a professional gambler who had a highly worked out system. He has long since lost it all on the horses and the new ways of calculating winning lines by the pools companies now make his old method no longer possible, but he does have a new one which he is sure will work – only it requires many mathematicians, a computer and a lot of money to try out. The pools company's permutation experts who called on him, he assures us, have told him he could be right too.

Less varied than the methods used for selecting the winning entries, were the hopes and expectations the sample had before their win. Although this aspect of the research did not form part of the statistical sample in the first stage of the work, the comments in the taped interviews certainly suggest that the majority had few ideas about what they would do if their coupon "came up". Like Vivian Nicholson, some of our

respondents did not seem to even believe that such large wins actually occurred – "it was all newspaper talk" – as one put it. Another winner commented:

"I think we was like everybody, we used to think it was a fix. I think everybody thinks that don't they? They do. You know, you used to read it in the paper and think it was a fix – they'd paid someone to say they've done that. I knew you could win two or three pounds, or perhaps a few hundred but never that much, we never dreamt of it, never talked about it."

So it is perhaps not surprising that many claimed they had no plans or even dreams about what they might do, particularly if they had any idea of what the odds were against winning. A typical, fatalistic comment of this sort was: "It never entered my head, I had no dreams, none whatsoever . . . I took pot luck, and if it come it come, that's all. We never had no idea whatever and never had no wish to do anything. Didn't know it would come." His wife added, however: "After it came we knew what we were going to do because we knew that he (the husband) knew a bit about building and that's what happened, you see?" So despite the initial lack of direction the couple did eventually develop some picture of what they wanted to do, and to remarkable effect, as the husband, who had been a builder's labourer at the time of his win, further explained: "We had a bit of land and a pair of houses, and we built them and showed a fair bit of profit. Then I bought eight and a half acres and built eighty-five houses on that and from there to another eighty-five. And from there to another eighty-six and eighty-six again, and that's how I've been going on. We're on 140 houses now!"

In marked contrast to those who had no real prior idea about what they might do if they did win, were others whose fantasies were often elaborate and specific, although in the event never fulfilled. Sometimes this is because the win, although large, just isn't sufficient to finance the scale of the fantasies themselves; an indication perhaps of the extent to which the pools may become the universal panacea for all our problems, and the focus and departure point for our day

dreams and psychological means of escape. One winner who, over twenty years ago, won an amount that today would be the equivalent of a £150,000 win, put it this way:

"I used to think anybody who does coupons was daft to say they didn'a think what they would do with the money. I had a lot of ideas, and I was a silly bugger when it all finished. My ideas, instead of being about getting a house and living decently, I had ideas of motor cars, a big house with thirty rooms and all this. After I got the money I started to realise that was way outside the thing altogether, it was going to cost practically all the money to live like that."

He explained that the capital outlay necessary to pay for all the things he had dreamed of would have left him very little after that to invest, to provide him with an income. So he settled for a bungalow and a smaller car, and through careful financial management kept the bulk of his capital for himself and his family.

Recalling these fantasies was sometimes quite amusing for the winners themselves and interestingly enough, in the case of one at least, the dreams eventually re-emerged more or less intact:

"I think all people imagine these things, I certainly did, you know – what it would be like when you had a fantastic amount of money – but you certainly didn't plan seriously. You know you'd sort of think, if I won the pools it's straight away a Rolls Royce and a trip around the world. Well actually it didn't come out like that. Soon as I knew we'd won it we started sitting down and talking and all the sort of fairy tale dreams like go out of the window when you realise it's there. I still do actually, think about the 'fairy tales', I mean I could go far on a castle with a river running next door to it, like. One of my ambitions is that – I've always been interested in fish, plants, and animals – and I've always said I fancy a trip down the Amazon or something like that."

In fact though, reality turned out rather differently: this winner bought a shop with the win very close to where he had been living before. In his case it was his wife who provided a restraining influence:

"I don't know whether I should go with him, I should have

to think about that one I think. I'm not very good, you know, with creepy-crawly things, animals, anything like that. I very likely would. I don't think I had any dreams myself, no: I didn't think it could happen."

Her hopes, what few she had, were more prosaic: "The mortgage was the main thing, and little luxuries we'd never been able to have: a washer, save me doing it by hand, different little things like that." We found in a number of other cases examples of wives proving a restraining influence: one man had always wanted a race horse but his wife "hadn't let" him buy one.

Very often the dreams were family ones: help for children trying to set up homes for themselves, or other relatives who might appreciate a little assistance. For example, one woman described her feelings as follows:

"I think we all have dreams about winning the pools. I used to dream I was going to buy my mother a house and buy my brothers a house and things like that. That's the only sort of dream I ever had. But nothing spectacular or anything like that. Both my brothers have got their houses now and my mum lives with me because my dad died. I've done that."

This winner and her husband had for much of their lives wanted a small market garden and the win therefore enabled them to realize this ambition, although on a scale previously quite unenvisaged. They in fact bought a 300 acre farm and now work it with the help of the previous owner's farm manager, who they kept on, and have developed a market garden as part of it:

"Well now, this farm, we've always wanted this sort of life, and never dreamed we'd be able to do it. We've got the market garden, we're building that up. But we've also got the farm and the house and everything to go with it. And my dream kitchen, I think every woman dreams of a dream kitchen. I have, you know, done all that. But that's all I ever dreamed of."

In view of the lengths of the odds against winning, it is perhaps not surprizing that most winners had few really well worked out thoughts about what they would do if they ever did win the pools, whatever their wilder fantasies might have

been. Some of the latter were also possibly too personal to reveal. Part of the sample won many years ago, when pools wins were not so frequent or so long established an event, and therefore there may have been less stimulus from the media and pools promotional publicity to create such fantasies in the minds of those who participated. Perhaps, too, people simply forget the dreams they had before their big wins, since to judge from the publicity material of the companies, they at least are in no doubt that the pools are the framework around which we all weave our dreams. However, two of the sample, if not dreaming about what they would do if they won again, retained the firm conviction that they would win a second time. They had both spent almost all the cash and so, perhaps, this belief acted as some sort of defence against feelings of bitterness that they had lost the chance of holding on to the money. And yet one of these – an ex-builder's labourer – showed little evidence of resentment. He seemed to feel that total conviction was an essential pre-requisite for obtaining another win. He had had no detailed ideas about what he might do prior to his real one when it occurred in the late 1940's: "None at all, I didn't make any plans about what I was going to do. I was only going to have a holiday as long as the money lasted." He decided eventually that the investments he had bought with the win were too heavily taxed and therefore did not provide an adequate income: "it would have been a good investment only the tax got too heavy: unearned income. Of course you know what that was, half of it went back in tax so I just suddenly thought, 'Oh sell the bloody lot and spend the money, it would be better'."

Being an early winner, he of course won before the pools companies set up their advisory services which were described in some of the earlier chapters. The whole question of whether winners in general hold on to the money from the win is discussed later. Here it is enough to say that the pools companies themselves said the service was started because winners in the early days were too often getting into difficulties through lack of business and financial experience. That this service has not always worked to the satisfaction of

winners can be seen from the accounts of Mrs. Nicholson and Richard Taylor. How successful is this service then, and how did the winners in our sample view it overall?

In order to obtain some information on the role of the football pools companies, we included in our interview schedules the question: "Could you briefly tell us what help or advice the pools company gave you concerning handling the money you won?" Forty-four per cent replied that they were advised by the pools companies, while eleven per cent reported that they were offered advice but refused it; and twenty per cent said that they were given no help or advice. The remaining answers suggested that the pools companies did provide them with some help, but it fell short of the full service now provided of a tax consultant, stockbroker, bank manager and solicitor, etc. This is probably because, as previously mentioned, our sample includes winners who won before the service was set up and it would appear from the replies that until the service was underway, assistance was offered at first only in a piecemeal and ad hoc way. For example, fourteen per cent of winners replied that the company representative told them to go to their bank manager or solicitor and follow their advice, or found them a bank manager and solicitor if they didn't have one already. Other specific help was mentioned too: two members of the sample stated that the company representative advised them "to look after" their families, and seven others – eight per cent – said that they were advised to consult the pools company or other experts before starting up in any business ventures. One winner commented that Littlewoods give

"every assistance under the sun. They look after it for three years and ask the bank for statements to make sure I wasn't doing anything. Before, people didn't take advice and blew it in two or three months: I should think any sensible person would want to take advice, especially if they are used to twenty pounds a week."

In the light of this, it is a little surprising that so many winners refuse the advisory service. Sometimes this is because the winner is used to handling money or has relatives or friends who they feel can do as well. For example, according

to one winner who was a farmer, the pools company "wanted to, but I told them I would deal with it. I trusted my bank manager." Or as another of our respondents explained:

"Well, through no fault of my own they didn't give me any advice, they offered it but I didn't take advantage of it. My son was training for accountancy and he knew of some portfolio manager to invest it, and only one fifth was invested at that time."

A similar point was also made by another winner, a woman who stated: "I have educated brothers and the pools people charge." Obviously the effectiveness of these family contacts is likely to vary considerably; they may indeed prove cheaper than and as efficient as the professional services arranged by the pools firms but we do know that sometimes they certainly have not been, and have been a factor in cases where winners have found difficulty in holding on to the proceeds of their wins.

Besides the question of advice on financial matters, we were also interested in how far the winners themselves felt that the pools companies had influenced their behaviour in a more general way: for good or ill. We therefore included on the interview schedules the question: "Do you think the football pools company influenced you to behave in any particular way?" The wording was deliberately kept open so as to prevent any suggestion to winners about how they might reply, especially as some winners had received their wins some years earlier and therefore might have had a problem in remembering what took place at the time. But despite the problem of recall, it is probably true to say that if the winners had felt any major pressure from the pools companies, they would not have forgotten this, especially if they felt it had been undesirable. Seventy-eight per cent of the sample felt that the company hadn't influenced them, seven per cent thought that it had, and a further seven per cent said they did not know (it was not possible to code the remaining eight per cent).

Of those who felt they had been influenced, most seemed to think it had been wholly beneficial. As one put it: "Yes they influenced me to look after it properly, taking their

advice"; and another: "They told us to be cautious"; and a third remarked: "Yes, this was a great help, very useful, very nice." One of the don't knows, on further reflection, made the same point:

"I don't know, it depends what you mean. They influence you to do the most sensible thing with the money, not to waste it, but invest it wisely. The pools people are definite about that. They tell you not to go astray and make a mess of it. If they smell a rat they soon tell you; if they thought you were being conned like with the begging letters."

On the negative side, one or two winners felt they were being pressurised over the publicity; one of our respondents stated that the representative "kept on" till he agreed. Another said: "They wanted all this publicity because there was an 'X' on the coupon, but there's no point in putting an X unless they are going to adhere to the rules." The whole question of publicity and the problems it brings is discussed in the final chapter.

The taped interviews tend to support the above picture; as one winner explained when he described his experiences with the advisory service:

"The gentlemen asked me what my views were and then they stressed several times that it was my money and I could do as I liked with it. And then, after I'd given them my proposals they said, 'Well this is what we propose; and as I thought theirs were much better than mine, I adopted what they suggested."

The plans involved providing a certain amount in cash and so much in investments, "because if anything happened to me in the first year I should have had to pay £160,000 in death duties." The advice is free initially, but as this winner further explained: "Of course when they start to work for you, you pay them you know: solicitors, stockbrokers, and all the other people, about five or six experts, managers of Trustee departments." And as Richard Taylor pointed out in the account of his win, the services can be expensive. Another criticism made about the service was that the panel had not gone to enough trouble to explain what was being discussed, and as a result, the winner's wife in particular had

felt very bored by the whole business and rather left out.

Apart from advice, the only other way in which undesirable consequences from winning large amounts were mitigated, was by limiting the size of the wins through the voluntary agreement of the pools companies to restrict the top dividend to £75,000. This was abolished in 1957 when the advisory service was introduced. In the early days, as one put it, they "gave me the cheque and said farewell." Today, at their most comprehensive, the pools companies advisory services almost resemble a mini-welfare state, with "social worker" company representatives, and services which promise to advise on, and help cope with all of life's possible contingencies after the full publicity potential of the win has been exploited.

What, though, do the winners buy with their new wealth? We attempted to answer this in two main ways: first, through an assessment of the consumption patterns of the pools group compared to those of the matched non-winning group: both samples were asked if they owned a whole range of possessions, from television sets and fur coats to swimming pools and private aeroplanes. And second, by asking two further questions in an open ended way: "What big things did you buy with the win?" and "Were there any other important ways in which you used the money?" These latter two questions were designed to gauge the subjective ways winners felt they had spent their wins. The objective questions had the additional purpose of acting as indicators of the life-styles of the two groups through the purchases they made. It was possible to group the answers given to the question on the "big things" that winners bought into sixteen categories, which are listed on the following page;
(the figures being the numbers of winners out of a total of eighty-eight who mentioned having bought a particular item).

What Big Things Did You Buy With The Money?

		Number of Winners Mentioning Item
1.	Bungalow, House, Flat	71
2.	Car	55
3.	Consumer Durables (e.g. televisions, record players, washing machines, freezers, etc).	33
4.	Furniture and Home Alterations/Decorations	32
5.	Holidays and Cruises	14
6.	Property (other than own house – mainly houses for relatives)	10
7.	Businesses	10
8.	"Things for the Family"	9
9.	Other/Miscellaneous	7
10.	New Clothes	5
11.	Caravans	5
12.	Nothing bought/Money left in Bank	5
13.	Farms/Land	4
14.	Investments	4
15.	Race Horses	3
16.	Boat	3

The table clearly suggests that "home centredness" was the major concern of winners. The most frequently mentioned item was house purchase, mentioned by seventy-one members of the sample, and the third and fourth categories – consumer durables, and furniture and home decorations and alterations – also illustrate this tendency. The figures should be treated with a little caution as they are based on open ended verbatim answers like: "A car, colour television, radiogram, bungalow and new furniture for the bungalow," rather than replies to individually specified items. Nevertheless, they broadly illustrate the overall pattern.

We also classified the replies to the second subjective question, and these are again listed out of a total of eighty-eight on the following page.

Were There Any Other Important Ways You Used The Money?

		Number of Winners Mentioning Item
1.	Travel, holidays, cruises	38
2.	Investments	23
3.	Helping Family	19
4.	No other important ways	18
5.	Other/miscellaneous	16
6.	Charities/helping people	11
7.	Buying houses for other people	10
8.	House alterations/furniture and decor.	7
9.	Buying house for self	6
10.	Buying other businesses	4
11.	Insurance for self and family	3
12.	Private medical treatment	3
13.	Car	3
14.	Not answered	3
15.	Enjoying myself/living well	2
16.	Women (!)	1

Travel, holiday and cruises are mentioned most frequently in reply to this question, a typical verbatim answer being, "We had a nice holiday in Italy and Switzerland." Some were more ambitious: "We went round the world on a boat to Australia and saw relatives we hadn't seen for thirty years," and another, younger winner said: "I saw the world, enjoyed myself, got drunk a few times, spent two months in Bermuda and toured the lower Islands of the West Indies. I also had two months touring Europe, and had at least two holidays a year, the normal things for anyone in my position." A widow also reported that she and her husband "just went on a wild cruise for four months on the 'Shannon' P and O liner."

Summarizing the significance of these two "subjective" questions, what perhaps is striking is the marked absence of unusual or idiosyncratic ways that the wins were used. Occassionally, however, the replies did suggest a degree of

individuality: one winner living in the East End of London paid the rents for all the people living in his street, the week he won, and the widow of another described how her husband had used his money to help people out: "A chappy jumped into the canal to save a child and he lost his wallet in doing it. My husband gave him some money. He helped lame dogs but I don't know how much was given away." Small traders also sometimes give gifts to their clients – one gave a chicken to each customer as it was near Christmas when he won – but these acts of generosity can sometimes misfire. A butcher, for example, said in a press interview that he would be giving away a joint of meat to each of his regular customers. His shop, not surprizingly, became inundated with customers, and eventually he had to close down because of the difficulties of getting customers to pay, although he had intended originally to keep the business going, (he was however, a non-respondent in our survey, so we cannot vouch for the veracity of the newspaper stories which reported this). A further reply to one of the subjective questions did provide a glimpse of an interesting transformation: "I went on a diet (presumably at a health farm) and bought new clothes." This winner now spends much of his time studying further education courses at his local college. Perhaps the most romantic way in which the win was used was by an ex-Royal Navy Petty Officer, who bought a £35,000, eight berth yacht which he lives on all the year round, cruising in the Mediterranean.

Concern for relatives was the other tendency which was clearly discernible from the replies to these two questions. Most of the "other properties" included in the list were bought for relatives, for example. And helping members of their family was also mentioned in various other ways: one couple said they paid for some relatives to return to England from Australia.

The overall theme of "home-centredness" may be slightly exaggerated however, as house purchase is advisable for pools winners for two or three reasons. First, if the family was living in a council house at the time of the win, the local authority may ask them to move in order to make the house

available for those more in need. Second, the begging letters, the stares from neighbours, the gossip and the requests for money may become just too much. For this reason, as one explained, the pools company representative suggested that it would be best for him to move away to an area where he was not known, "if he could bear to give up the things he was used to." Additionally, a financial advantage of moving is that a long mortgage on a new house can be offset against the high tax rates on unearned income. But for whatever reason, most winners felt that the win had meant, above all, a new house and this was also reflected in the furnishings and decorations of the houses themselves. Often on visiting a winner's home, the period of the win is encapsulated in the purchases they had made: fifties furniture and decor for winners who won at that time, and sixties and seventies styles for later winners.

Comparatively few mentioned investments in their answers to the questions on ways in which they had used the win. This was probably because the one immediately before on the interview schedule, specifically asked for details on this. The table below summarizes the full responses obtained to this question:

Amount of Win Invested

	Number	*Percentage*
Under ¼	6	7%
¼ – ½	8	9%
½ – ¾	31	35%
¾ plus	17	19%
Not known	26	29%
Non coded	1	1%

Seventy-seven per cent of the sample said they had invested a half or more of their win. Most of these were stocks and shares, or Building Society and Local Government bonds. Of course the "not known" category includes those who declined to provide information on their financial arrangements, but it also included some who claimed that they did

not know what the total amount invested was. This is not so implausible as it sounds since, if the advisory panel invested the win, the way this was carried out may have eluded the recipients themselves. One or two winners, as we have seen, claimed that the financial discussions with the panel had been very boring, and one also complained that the advisors should have done more to explain what was happening. Also, in the tape-recorded interviews, respondents occasionally remarked that they would have liked to have actually seen the win in cash at some stage to have enabled them to grasp the magnitude of the win. In one instance it did not dawn on the winner until the bank manager explained to her that if she had wanted all the money in cash, there and then, the bank wouldn't have been able to supply it, as they didn't have that much money in the vaults. The winners who had come from working class backgrounds may have been only used to receiving their wages in cash at the end of the week, and may not even have had a bank account. Therefore, the abstract discussions about investments and bank accounts were likely to be very foreign to their own conceptions of and experiences with money. It is why too, in at least one case, the pools company had to stress repeatedly that the win belonged to the winner concerned.

The extent of home ownership amongst pools winners comes out even more clearly in the following Table, which summarizes answers to a specific question directly on this topic:

Home Ownership

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Rented	3	3%	47	53%
Owned	85	96%	39	44%
Other	1	1%	3	3%
Total	89	100%	89	100%

The types of property most preferred by winners were detached houses; owned by forty per cent of the pools group, compared to five per cent in the comparison group. Bungalows were mentioned next most frequently: by thirty-three per cent, compared again to only six per cent of the matched group.

Over the whole range of consumer and luxury possessions we asked about, as shown in the Table below, pools winners showed a high incidence of owning such possessions, compared to the non-pools group.

Do You Own the Following?

	Pools Winners	*Comparison Group*	*Pools Winners*	*Comparison Group*
	Number		*Percentage*	
Telephone	83	57	93%	64%
Tape Recorder	37	22	42%	25%
Stereo Record Player	49	27	55%	30%
Colour TV	33	7	38%	8%
Colour TV Rented	24	19	27%	21%
Typewriter	43	26	48%	29%
Cine Camera	41	11	46%	12%
Piano	38	33	43%	37%
Electric Toothbrush	3	4	3%	4%
Powered Lawn Mower	63	23	71%	26%
Deep Freeze	31	7	35%	8%
Coctail Cabinet	37	17	42%	19%
Central Heating	69	27	78%	30%
Motor Boat/Sailing Dinghy	13	1	15%	1%
Caravan/Dormobile	18	4	20%	4%
Fur Coat	47	20	53%	22%
Pony/Horse Personal Use	12	3	13%	3%
Race Horse	4	0	4%	0%
Yacht	1	0	1%	0%
Private Swimming Pool	3	1	3%	1%
Private Tennis Court	3	0	3%	0%
Private Aeroplane	0	0	0%	0%
Dishwasher	9	1	10%	1%
Total Items Owned	661	310		

The patterns of ownership – as distinct from the quantities possessed – were not markedly different between the two groups however, and therefore do not reflect radically different life styles. For example, neither group contained anyone who had ever owned a private aeroplane, and only three had ever owned a private tennis court or private swimming pool. But the middle range of items, such as colour television sets, central heating and powered lawn-mowers were owned two to four times more frequently by the pools winners compared to the non-pools group.

The picture on car-ownership and holidays is similar: more of what most people seem to regard as the good things of life, without any radical departure from the norm. Seventy-one of the pools winners had one car or more (three being the most owned), compared to forty-four in the comparison group. And the models, whilst obviously newer, were rarely really extravagant: only one Rolls Royce and one Bentley, and no Maseratis or Ferrarris. A list of the makes of cars owned, based on a one-in-three sample of the pools group, and a one-in-two sample of the comparison group, is provided on the next page.

The average age of all pools winners' cars in 1972 was two years, compared to five years for the non-pools group. And the average second-hand value of winners cars was £1,224 (at 1972 prices), in contrast to only £492 for non-winners.

Seventy-five per cent of the pools group took a holiday in 1971, compared to sixty-nine per cent of the non-pools group, but the number taking a second holiday was twice as high – twenty-seven per cent to thirteen per cent. It would seem, too, that there is no great taste for foreign holidays, after the initial travel and cruises following the win, since the numbers taking holidays abroad in 1971 were virtually identical: thirty-five per cent for the pools winners and thirty-six per cent for the comparison group. This is probably due to both the conservatism of the pools group and their age. Some of the respondents were elderly by the time we interviewed them for the survey: the average for the two samples was fifty-eight, and thirty-one per cent were sixty-five or over.

List Of First Cars Owned
By Samples Of The Pools And Comparison Groups

Pools Group	*Comparison Group*
1959 Morris Minor 1000	1958 Ford Cortina
1964 Rover	1972 Triumph Toledo
1962 Morris 1000	1968 Mini
1968 Austin 1100	1972 Datsun Cherry
1971 Hillman Avenger	1968 Ford Escort
1970 Triumph 1300	1965 Morris 1100
1966 Land Rover	1964 Anglia
1969 Wolseley	1970 Austin 1300
1972 Jaguar	1968 Singer Vogue
1955 Rover	1972 Renault 16
1969 Rover 2000	1973 Hillman Hunter
1972 Hillman Hunter	1964 Ford Zephyr
1972 Rover 3 Litre	1971 Simca 1100 GLS
1967 Ford Corsair Automatic	1970 Hillman Avenger
1967 Riley Automatic	1958 Austin A55
1971 Wolseley	1964 Ford Saloon
1972 Volvo	1972 Ford Escort
1970 Ford Cortina	1967 Morris 1100
1970 Humber Sceptre	1971 Triumph 1300
1971 Renault	1970 Vauxhall Viva
1971 Rover	1968 Ford Cortina
1972 Audi	1963 Ford Anglia
1971 Vauxhall Viva Automatic	
1972 Daf Estate	

The following is a list of the places where a one in three sample of the pools and non-pools groups took their holidays in the year previous to their interview:

List of Places Where Samples of Pools and Non-Pools Groups Took Their Holidays in Year Previous to Interview.

Pools Group

1. Cadiz, Casablanca, Tenerife and Madeira (Canary Islands).
2. Wales.
3. Sidmouth, Bournemouth.
4. U.S.A./Canada, Tenerife.
5. Bournemouth, Kent.
6. Isle of Wight.
7. First Class World Cruise (Australia).
8. Greece. London.
9. St. Ives, Cornwall.
10. Portugal. Cup Final in London.
11. Touring in England and Wales in a caravan.
12. Southport.
13. Bombay, India (Respondent originally from India).
14. West of Ireland (Respondent living in Ireland).
15. Canary Islands, Cornwall.
16. Boscombe, Bournemouth.
17. Scarborough, Coach Tour.
18. European Tour (Yugoslavia, Greece, Italy, France).
19. Easter Ross, Scotland.
20. Majorca, Holland, Chester.
21. Ireland, Wales.
22. Touring England (Visiting friends in Southampton).

Non-Pools Group

1. Eastbourne.
2. Spain.
3. Germany, Holland. Lake District.
4. Yugoslavia.
5. Staying with daughter in Scunthorpe.
6. Llandudno, Wales.
7. Switzerland.
8. Scotland.
9. Morton, Dorset.
10. Holiday Camp at Osmington Bay, Weymouth.
11. U.S.A.
12. Lowerstoft.
13. Llandudno.
14. North Wales.
15. Majorca.
16. Margate.
17. Cycling Tours: Thames Valley, Scotland, Wye Valley, Jersey.
18. Australia.
19. Cornwall.
20. Jersey. Caravanning in Wales.

There is nothing particularly remarkable about the kind of places that pools winners choose for their holidays; perhaps there is a slight tendency for them to go a bit farther afield than they would have done without their money (the Canary Islands appears to be more popular amongst them for example). They also appear to choose the more "respectable" home holiday resorts, places like Bournemouth and Scarborough.

The objective questions also confirmed the common response of giving some of the win to relatives. Eighty-nine per cent of the sample agreed that they gave part of their win to relatives, and only one said he had not (the remainder either refused information on this or said they did not know). Amounts, however, varied considerably: amongst the largest sums was one given by a winner who gave to his two daughters, a total of £75,000, in this case one half of his win. Another particularly large share-out consisted of giving "twenty relations, aunt and cousins near enough £1000 each," together with £2,500 to the winner's mother, £15,000 to his mother- and father-in-law and £7,000 each to a brother and two sisters-in-law, all of which amounts to £59,000. A more typical amount mentioned was the total of £10,000 that one of our respondents gave to his sister and brother-in-law, half to each.

Friends of winners, however, fared much less well: only thirty-three per cent of the sample gave part of their win to friends, compared to fifty-four per cent who did not. The remainder either said they did not know how much they had given away, or refused to give information on this question. Amounts tended to be smaller, the largest single payment being £10,000. More typical sums were in the range of £50 to £250 to "four people who worked with me." Sometimes amounts were "loaned" to friends which were never repaid, and which the sample sometimes included in the amounts they considered they had given away. Of course one key factor in the amounts given to friends is the size of the win itself. Those winners who won around £130,000 at today's values, the minimum amount required to be included in our survey, would obviously be in a less favourable position to give away large sums whilst still retaining sufficient to provide an investment income for themselves and their family, compared to those winners who won over £400,000, the highest amount won in the period we looked at.

Other restraining factors are, of course, taxation and death duties. Those winners fortunate enough to have secure marriage relationships can take advantage of this by "giving" half the win to their spouse to split the investment into two

smaller amounts to diminish the taxation on the investment income from the total. It does require a certain vote of confidence in the marriage, however. Similarly, death duties loom fairly large for the elderly winner's family. Large amounts of the win may have to be given away quickly to maximize the chance of the winner surviving the seven years (formerly five) necessary to escape the high death duties that otherwise would become payable by the beneficiaries. Indeed, because the investment income from the win is taxed heavily even if the winner does survive after he has given away the usual amounts to his family or bought a new house and car and taken a holiday, the amount he has left as income may not be substantially greater than the one he was earning when he worked, especially if his win was close to the lower end of the winning range with which we were concerned.

Thus, sentiment is perhaps not the only factor in determining what amounts should be disposed of as gifts, and even that is likely to be tempered with caution for the sake of family peace; as one winner explained: "We treated them all the same so you wouldn't be afraid of one saying 'Well they gave me so much'; I mean, it would cause a lot of unpleasantness wouldn't it? . . . I shouldn't think you'd be able to speak freely in between and, you know, if they all met it would be an awful job in case one let out what the other had and they were different." Her husband added: "it would be bound to come out in time."

The same man went on to explain how he decided to whom to give what amongst his friends:

"You can pick say a dozen out, really genuine friends, I mean really genuine ones; you knew that because you had worked with them for years and years and years. The only thing is, I sent this money to them on the quiet because I couldn't treat everybody the same, I mean there's thousands of chaps up there I know besides those . . . but they're not really my closest friends, genuine friends and it was the same with the wife you see."

However, how family relations and patterns of friendship are affected by the win, are discussed in the following chapters.

CHAPTER X

"It Won't Change Us"

"I know that's like a music hall joke, you see it in the papers and on the television, 'if you won some money you wouldn't change your lives', and everybody has a good laugh about that. But to me it's important you know."

Why do so many winners feel that it is neccessary to declare: "It won't change us?" Perhaps it is partly because the public and press image of pools winners is one of wild spending, unhappiness and the eventual loss of all the win, and naturally the new winner would wish to dissociate himself in advance from so unfortunate a label. But the tape-recorded interviews suggest that it is not only a question of press sensationalism: for some, the shock of the win is such that it poses a threat to the very identity of the winner himself, his whole personal world becomes threatened. An illustration of this point can be seen in the comments of one factory worker who won over twenty years ago:

"I was kind of frightened when I got the money, maybe you can't believe that. But if you were in the habit of just getting say four or five pounds a week – that was about the average wage you asked for at that time – if you were in the habit of getting that and then you are suddenly asked to go into a bank and you find you have a cheque for seventy or eighty thousand pounds, well it's a big hit . . . I lay on my bed that night and I drank a whole bottle of whiskey between say eleven o'clock and half-past four in the morning, and I never turned a hair I was that damned tense."

It is not necessary to be middle-aged to react in this way; younger winners too, experienced similar feelings of total disorientation by the impact of the experience, especially if

they were beginning to achieve some of the goals they had set themselves: perhaps some progress at work, enough saved for a deposit on a house, and one or two items on hire purchase. For someone in such a position, winning is not necessarily wholly welcome:

"... it hits you hard at the time. You begin to think, like, 'Is it going to change me? I was happy where I was.' I didn't want this to happen, I wanted to be as I was, as I was brought up to be. I would sooner say to somebody, 'Here's the cheque,' take it , if I thought it was going to change my life." This winner went on to explain how he had been in line for promotion and had just arranged to move to a more suitable house, and having come from a poor background, he had naturally felt a good deal of satisfaction about the course his life and that of his family had taken. Suddenly, with the win, all that was threatened.

This sense of disorientation was perhaps more fully expressed by another young married working class winner, who explained:

"I think it's wrong, winning large sums of money ... it disrupts your life too much and creates too many problems. It sort of alters, it's hard to describe, your plans if you like: you are working towards a goal and then everything falls into your lap. You can't seem to grasp the fact that you're rich. I suppose it all boils down to getting a certain amount of satisfaction out of anything. I don't regret it, like, and I wouldn't give it away, but at the same time it sort of destroys your ambition and you have to start all over again. That's the way I found it anyway ... Now I've got to the sort of stage where I can start to do something new and perhaps become ambitious again, but at the start it knocked the bottom out of any ambitions you'd got."

Initially, then, there is a need to deny the force for potential change that the sudden riches represent, at least for all those with average income or less. But the win has the capacity for even further transformation, since it touches on almost all the central areas of an individual's life: work, family relationships (through suddenly becoming the rich

relative for every member of the family), friendships, social position and leisure activities. It also creates the very real opportunity of being able to live out many of one's wildest fantasies and dreams.

These factors are perhaps only dimly sensed beneath the immediate shock of the win itself, and it may be some while before the effects of the win may be articulated, if ever.

Yet, as the early chapters showed, the win affects people in a variety of different ways. There are many reasons for this: the age at which a particular individual or couple win, their personality, their social background and the reactions of others: family, friends and neighbours. For example, someone with a phlegmatic temperament who wins fairly late in life, may not be much affected by the win: "Once you're into your forties, you are pretty set in your ways and you don't change much", as one put it. It may mean in those circumstances simply a more comfortable or even luxurious retirement, especially if the person concerned was already fairly well off. They may still say: "It won't change me," but with perhaps less urgency.

One way in which the win may bring about changes is by giving some winners a greater sense of independence and self-confidence. One of our sample made this point forcefully about the person with whom he shared the win. They won "first" and "second" dividends in the same week and met at the joint reception for the presentation of their cheques. The co-winner appeared at this function to be a very shy, self-effacing and even timid man, for whom the briefest conversation was evidently a painful experience. Sometime later, after they had both become more accustomed to their new wealth, the first winner learnt that his shy colleague had decided to call and stay with him for a week or so. In view of his personality it promised to be a rather long week. However, when he arrived, he turned out to be extremely confident and out-going and very good company indeed, much to his host's surprise.

For the pools winner who is working at the time of the win, one of the earliest decisions to be made is whether to

carry on in the same job. Often the opportunity to give up work is the main reason for doing the pools in the first place:

"The only thing I thought of was getting away from work as a bus driver, that was one of the main things, and possibly purchasing a small business; I didn't want anything spectacular."

A similar sentiment was expressed by another member of the sample, who said to his bank manager when he went to see him about the win: "You can do what you like with us, so long as we don't work again."

This would seem to be a very common reaction, since seventy per cent of the pools group were working at the time of their wins, but only seventeen per cent after it; and the latter figure includes both those who had spent much of their money and had been forced to return to work through economic necessity, as well as those who initially gave up work, but started again sometime later as something to do.

The Table below gives the full picture:

Number in the sample employed before and after their wins.

	Before		*After*	
	Number	*Percentage*	*Number*	*Percentage*
In full-time employment	62	70%	15	17%
In part-time employment	6	7%	7	8%
Unemployed	1	1%	1	1%
Retired	6	7%	37	42%
Housewives	11	12%	16	18%
Other	3	3%	11	12%
Not coded	0	0%	2	2%
TOTAL	89	100%	89	100%

Partly, of course, the number of those not working amongst the pools winners is a reflection of their average age: they were often interviewed for the research project at a time in their lives when they would have been retired anyway. This can be shown by the next Table which contrasts the employment situation of winners with that of the group of

non-winners with whom they were matched for comparison purposes, but even so the number not working is still higher for the pools group.

Numbers Employed in the Pools and Comparison Groups.

	Pools Group		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
In full time paid employment	15	17%	41	46%
In part time paid employment	7	8%	14	16%
Unemployed	1	1%	3	3%
Retired	37	42%	23	26%
A housewife	16	18%	4	5%
Other	11	12%	4	5%
Non coded	2	2%	0	0%
TOTAL	89	100%	89	101%

Many of the winners replied to the question of why they stopped working with a simple, "because I won the pools," as if any question of work being anything other than a source of income was quite incomprehensible. One put this view particularly succinctly: "Because I now had enough money to live on without working for it." But even if a winner wants to keep the job he had at the time of his win, pressures build up to make staying on difficult, as Mrs. Baker found with her nursing. Taxation levels, too, for those with large amounts of capital are no encouragement to remain employed:

"If I had stayed on working, they would have taken off as much in tax as what I was going to earn if I had stayed at work, so I didn't see any point in working for the Government. I'm not very keen on the Government anyway," was how one described the situation. An alternative factor, which tends to push winners into giving up work, is the attitude they feel others may have to their continuing in work. This factor emerged when we interviewed one member of the sample

shortly after her win:

"Well my husband won't work, not at the moment anyway. He'll have a rest first and if he gets a bit restless, well, he can find some little job to do. But he won't go back to the works anyway. Well, it wouldn't be nice, would it: winning all that money and going back to work? I mean, people do talk, don't they? Not that I would mind, because I thoroughly enjoyed my little job that I had, and I was sorry to lose it, but I can't very well, can I, go to work when I've got all this money?"

To some extent this may well be a rationalisation, but in areas where work is scarce, it does represent a real enough attitude.

Given that these factors more or less oblige the winner to give up his current occupation, premature retirement is likely to be one of the early changes which will occur in his life. And if, as Graham Eastcote described, the new winner then starts to feel restless and in need of some sense of purpose in life, the only alternative is to become self-employed or to start some other form of business activity. Those winners who do acquire businesses, choose grocery or newspaper shops, farms and small holdings, public houses, fish merchants, transport and property businesses. One winner held a small electronic partnership, but in general none were on a very large scale, seldom employing more than three or four staff. Nor was there always much attempt to run them profitably. As one early winner, now retired, put it:

. "The shop was O.K. We didn't make much money out of it, it was just something to do until the boys grew up . . . The bank suggested taking the shop; they said I needed an interest. I was only thirty-one at the time, so I took the shop."

Other winners have been less fortunate and found that the businesses they have started have cost them a good deal of money. One in particular could make this point from his own experience, having lost substantial amounts in one or two business schemes, before finally managing to run one successfully: "People who win money would do well to keep out of business, unless they've a business already and want to boost

it up a bit possibly."

But the new business can also be a very welcome change and a great new source of satisfaction. Paradoxically, it may be the first time he or she ever experiences a sense of accomplishment, if the jobs held prior to the win were routine or boring, and had been accepted only through economic necessity. Choosing an economic activity can give a sense of achievement, and this is perhaps best illustrated by the way one described a typical day in his life, spent on the pig farm he bought with some of the money from his win:

"Well . . . now you get this morning, Wednesday. I normally rise about seven to half past, I'm never after half past: well, I don't rush about, especially at this time of the year, winter time, and we have breakfast, but I'm generally out say about eight-thirty. I've no help, no hired help, I am doing everything myself; I'm just hard working now. I've just got enough work to do, enough to keep me busy, but yet I'm not tied down; if I want to go away, I'll get someone in to see to things, see. You get a day like today: I went out this morning at eight-thirty, fed the pigs, which takes just over an hour. I keep them well cleaned, I always take the dirt out almost every day, they're as clean as everybody's pigs mine are. And so I go around, feed the pigs, see that they're cleaned up all right and then a few odd jobs, there's always jobs to do on the farm, little odd jobs . . . I've got two large broiler houses and I've got a chappie now that rents them for pullet rearing. He rears pullets for his own use, for egg-laying purposes you see. This current batch of chickens went out last Thursday and yesterday, and this morning we've been cleaning out the places and they use my tracks and trailer: it wasn't a very busy morning this morning so I had a couple of hours work on the tractor with them, not hard work, just driving the tractor and taking the litter away into my neighbour's field. I came in for lunch just after twelve, and then it's our weekly market in town, which is six miles away, and I always go and check on the pig prices and so forth, you see. And we came back – the wife went with me – came back about three o'clock this afternoon, and put me feet up until just after four, and then went out and started cleaning

up again; and it was just after five when I came in after the pigs, and that's it."

But the change from worker to businessman is not always so smooth, especially if it means employing others. One winner – an ex-bus driver – found that being a businessman had made him harder:

"Oh I'm not so happy-go-lucky as I was, you know. I'm a bit more of a hard character, but you get that knocked into you . . . At one time I wouldn't like us to pay off men, but it doesn't bother me now, 'cos I can't be responsible for the country's unemployment, you know, so if there's no need for them I just pay them off. But at one time I was always very reluctant to do that . . . you just can't do it."

It would seem, then, at least in the work aspect of a winner's life, that a good deal of change is inevitable. As one winner's wife pointed out, when her husband expressed regrets that he had just walked out and left his firm and workmates at a time when they had a lot of work on: "When the man from Littlewoods comes, you just gotta go."

What are the other changes that result from the win? As Part One of the book suggests, it has great significance, not just for the winner, but also for his wife, children and all his relatives; relationships with friends and neighbours may also change. In a way, winning the pools must be a bit like a family squabbling over the last Will and Testament of a deceased rich relative, but without a corpse! And for the winner himself like having his own will read. The win may bring more changes for the children of winners than for the recipients of the win, provided, of course, the latter receive the money early enough to use it to influence the upbringing of their children. The most obvious way in which this happens is through private education: thirty per cent of the pools group had paid school fees at some time, compared with only nine per cent of the comparison group. This was despite an average age of forty-six at the time of the win, so that many in the sample would have had children who were already too old for school. Not all winners arrange private education for their children, however. One remarked that his were working class and were going to stay working class. And

as David Llewellyn's account illustrated, winners are sometimes reluctant to send their children to boarding schools, as they may see it as parental rejection. One couple said the pools representative had warned them of this problem. Even if the children do not go to private schools, the difficulty is that they see their father not having to work and expect, perhaps wrongly, that there will be sufficient money for them not to have either. Thus, they may show a good deal of reluctance to study seriously at school or think too much about a career.

The win does not seem to have much effect on parental desires to have more children, which the extra money would easily enable them to afford. The number of children in the comparison group was seventy-two, as against seventy-one in the pools group. As the average age of the respondents was forty-six at the time of their win, this would diminish any tendency for wealth to increase the number of children. Neither was there much difference between the two groups in the number of children, or relatives, living at home, so in this area at least, there is little variation in the lives of pools winners.

Where change does occur is in the number of "domestic helps" or "personal servants" employed. Four per cent of the non-pools group employed people in this capacity compared with twenty-seven per cent of the pools group, although none "lived in" as part of the household, and only three per cent had two "servants"; none had more, the remainder having one only. The term "servant" is therefore, something of a misnomer, insofar as it implies the nineteenth century idea of large numbers of maids and others actually sharing the household of their employer. Most of the people employed by pools winners worked on a part-time basis, either doing cleaning work within the house, or gardening.

What of friendship and neighbourhood patterns; are these changed by the win? Richard Taylor's and David Llewellyn's accounts suggest they are, as do some other winners, at least at the outset: a woman winner, for example, mentioned one way in which relationships can be affected.

"I had quite a good few clothes of my own, you know,

before the win and of course I bought quite a few clothes and gave a lot away. Well, I found two or three of my friends used to come on the quiet, maybe once a month: 'Oh can we have a look at your clothes, Hazel,' you know. Well, I'd take them up and, 'Can I try this on?', and there would be all four of us in the room trying clothes on. And then one would stand in the mirror and say, 'Oh this just fits me, I wouldn't mind this,' and I'd say, 'Go on, you can have it,' and give it to them, and then you'd never see them for a few months again. Now I've stopped all that, I give to someone that's really needing something, a poor person that really needs a few clothes."

For those who do not move to a new area, sensitivity to local sentiments may enable them to ride out the early period and return being an ordinary member of the neighbourhood community:

"This is a mining area as well, you see. Well, I mean I would go up in the village with my overalls on and wellington boots, anything like that . . . and I know that some people, who didn't know me, they've said 'Pools winner? He looks like he ain't got two half-pennies for a penny.' But I'm sort of one of the community. I mean, I don't go about dressed in rags, but people know me, I've worked hard as anybody, as far as that goes, and I think I'm well liked really – bloody hope so."

In the main, the survey findings do seem to support the comments of the above respondent, whether the winners move away or stay in the same area. That is, there does not appear to be a marked difference between the experiences of the comparison group and those of the pools group, as measured by the questions concerned with relations between friends, neighbours and relatives. For example, ninety-one per cent of both samples said that they found people living in their area were friendly towards them.

The pools group also appeared to feel less isolated from friends and family than the comparison group. Seventy-two per cent of the former agreed that they saw as much of their relatives as they would like, compared to sixty-two per cent of the latter, and, similarly, when asked the same question about "old friends", fifty-seven per cent of the pools group

said they saw as much of them as they would like, compared to forty-nine per cent of the non-winners group. These findings are not consistent with the newspapers' myths of pools winners losing friends and falling out with their families, at least in any long term sense. And inasmuch as pools winners are likely to have more time to visit friends and family (and more money for travelling to and from their homes), this finding seems plausible.

It is, however, when the amount of *communication* with neighbours is examined, that some support is found for the idea that isolation might result from a win on the pools, as the table below shows.

Did You Talk, Including On The Telephone To Any Of The Following People Yesterday?

	Pools Group		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Relatives (non household)	40	45%	50	56%
Friends	49	55%	56	63%
Neighbours	27	30%	53	60%
Spoke to no one	16	18%	11	12%

There are at least three possible explanations why pools winners talk less to their neighbours than the comparison group: first, they may feel awkward or socially out of place with the affluent, middle class in their new area; second, they may be rejected by them if they do attempt to make social contacts; and third, it may simply be that the majority of winners have moved from more gregarious, working class people to less neighbourly, middle class ones. All these possibilities are suggested, for example, in the following remark:

"We don't neighbour, but I suppose if you wanted them, they'd be there: you know, emergency or something like that, I think. But we know they don't want to neighbour, so we just accept it; they don't come round and we don't go round. We've invited them, but they've never come, so therefore we knew then they wanted it that way."

A further possible reason may be that, secure in their increased affluence, winners may simply feel less need for social support from neighbours, especially if they have experienced embarrassment or envy from these quarters. In such instances, they may well become more independent and come to rely almost exclusively on their immediate family for social support.

One facet of the lives of pools winners where change might be expected is that of leisure activities, and, as we have seen from the expenditure patterns of the two groups, they did tend to own more boats, race horses, stereo equipment, etc., although not to as marked a degree as one might expect. Similarly, with holidays, the patterns were not strikingly different from those of the non-pools group. As one winner in his forties put it with a laugh, and he could have been speaking for many others: "I think you picked a pretty dull couple." His wife added:

"Well, some people are more go ahead than others and they're in all the activities that are going, aren't they? But I like evenings quiet at home. I watch television quite a bit. We've a nicer house and a nicer garden and that, otherwise there's not much difference, is there?"

The couple added that they had bought a caravan, which they keep in the country and which in the summer months they visited for a few days from time to time. However, despite the overall average age of the sample, nineteen per cent of the pools group mentioned some participant physical recreation, such as bowling and golf, as their "chief leisure activity" compared to only eleven per cent of the non pools group. Only six per cent of the pools group, compared to ten per cent of the non-pools group, mentioned television as their chief leisure activity. For both groups, gardening was the most mentioned of all: twenty-two per cent for the comparison group and twenty per cent for the pools group. In some ways, it is perhaps surprising that the figure is not higher for the pools group, since the overwhelming impression from visiting all the homes of the pools winners is one of immaculate garden after immaculate garden, but perhaps in some cases, at least, professional part-time gardeners are

responsible for this! And, of course, winners do have more time to spend on this pastime, even if they do not regard it as their chief leisure activity.

There is something of a class dimension in the leisure activities of the sample we studied: six per cent of the comparison group engaged in middle class pursuits, such as golf, compared to seventeen per cent of the pools group. For working class leisure activities (e.g. bingo), the figures were nine per cent for the comparison group, as against three per cent for the pools group. The overwhelming majority of activities – seventy-nine per cent for the non-pools group and seventy-six per cent pools winners showed no distinctive class characteristics. Thus, although there is some tendency for an increase in middle class leisure patterns for pools winners, this is not a particularly marked phenomenon.

Travel showed a quite distinct class trend, as the table below shows:

First Class Travel

	Pools Group	*Comparison Group*
Rail	25%	3%
Sea	29%	7%
Air	15%	2%

Interestingly enough, only with rail travel was there a significant difference between working class winners and middle class winners on the extent to which they prefered to travel first class: thirty-one per cent of middle class winners travel first class by rail, compared to twenty-one per cent of working class winners.

Another area explored, indicating change in the life style of pools winners, was that of newspaper readership. Winners were asked which newspapers they read regularly, both currently and before their pools win. As it was felt that memories might be unreliable on this point, the question was also asked of the non-pools group, so that comparisons could be made both "before and after" as well as with the matched group. With the daily papers, few differences emerged before and after the win. Two per cent of the pools winners read the

Financial Times before their win compared to six per cent after it, and the one member of the group who read the *Daily Worker* (later of course the *Morning Star*) stopped doing so after his win! Other than that, there were no significant shifts reported on daily newspaper readership.

The patterns of readership of the pools group, compared to the non-winners group, are more difficult to assess, since the amount of grammar school secondary education of the latter was larger than that of the pools group. This probably means that they had patterns of newspaper readership which were untypical of those of the winners before their wins. Overall, the tendency appeared to be towards a greater amount of newspaper readership by pools winners for almost all newspapers, reflecting, of course, their ability to afford more; but the tendency was most marked with the popular rather than the quality papers, except for the *Sun.* The figures are given in the table below.

Newspaper Readership (Dailies)

	Pools Group		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Sun	13	15%	20	22%
Daily Express	36	40%	26	29%
Daily Mirror	38	43%	27	30%
Daily Mail	18	20%	10	11%
The Times	5	6%	2	2%
The Guardian	4	4%	4	4%
Financial Times	5	6%	0	0%
Morning Star/Daily Worker	0	0%	2	2%

Newspaper Readership (Sundays)

	Pools Group		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
S. Telegraph	4	4%	5	6%
News of the World	36	40%	35	39%
Sunday Times	11	12%	10	11%
Sunday Express	36	40%	34	38%
Sunday Mirror	39	44%	21	24%
Observer	5	6%	12	13%
Sunday People	32	36%	23	26%
Other	8	9%	8	9%
No Sunday Paper	8	9%	2	2%

What of the pools winner's overall *social* position in society; how does that change? This, of course, largely depends upon the way it is assessed, and the whole question of how social classes may best be defined is an area of voluminous discussion in the social sciences. But, insofar as almost all of the determinants of class and status have some relation ultimately to the economic situation of the groups individuals being classified, inevitably , winning large sums of money on the pools has very large consequences for the social position of the recipients. If, for example, property ownership and the possession of consumer durables and other goods are used as the main criteria of social status, then, as we have already seen, there is a generally "upward" trend in these areas.

Similarly, a radical alteration in class position occurs for those winners who switch from being employees to employers; this too was noted earlier, and is a transformation of which the winners concerned are likely to be quite aware. One of the sample replied, when he was asked if he thought his "social standing" had changed: "I think it has. Well, being in business puts you in a different class, doesn't it? Mostly the workmen call you 'sir' and all this sort of thing . . . You can't be so matey with them as you've been, except personal friends."

And if the general life-style of the pools winners is used as the main indicator of their overall social position, the list of possessions suggests, with its preponderance of home-centred items – central heating, powered lawn-mowers, freezers – a middle class way of life, rather than an aristocratic or "playboy" one. Neither do the working class winners show much inclination to maintain their distinctive class life-style. There are exceptions. however: one winner, for example, continues to live in his council house and spends most of his time at the races; he summarized his response to his win by simply stating: " Nothing has changed; I'm still the same."

This relative lack of changes is partly determined by the actual net income levels obtainable from the investments of a pools win. Today a £200,000 win carefully invested would probably return a gross yearly income of £20,000, but the figure *after tax* would not be more than about £6,000. So, if the socially aspirant winner wishes to keep his capital intact, he is more or less restricted, at the most, to a middle-class style of life, unless he moves abroad, as a small minority do. However, the larger wins do enable some winners to adopt an upper class mode of living, but even winners in this category tend to adopt a lower-middle class style of life. Thus, insofar as the sample are picked more or less at random from the total population, it would seem the British are an essentially petit-bougeois nation.

Even the middle class embrace, at least at the formal level of dinners and dances and "social functions", may be something of a shock, as one of our sample described, when she was asked if her social standing had changed:

"Oh yes, I mean, I've been invited to places I'd never, people would never, dream. I went to a Mayor's Ball, I was invited to that and I've been invited to be guest of honour, you know, guest of honour to lots of different things. I haven't taken them all on because. I mean, let's face it, it wasn't me, it was just to say, you know, I've had the pools win. But the genuine things: I've been on carnival things and I've been to blind babies do's and things like that. I'll go and do anything because they are genuine. I went to that Mayor's do, I will admit it, because I've always

wanted . . . you know, when you see it on the television: 'Oh yes, oh yes, here comes Mr. and Mrs. So and So,' and that was the biggest eye-opener I'd ever saw. My God, the wife-swapping and all the things that goes on. I think at the end of the night there was only Andy, my husband, and I still together. It shook me solid, you know, things like that. They were all the mayors from all over you know, it was a big social do for them. And the Councillors and all that were invited, why the heck I was invited I don't know. But we was invited and I went to it. But as I said there's been a lot of functions you could go to. I've been made some vice-president over some cricket club, well I've never been. I mean, I'd never been invited before and I don't suppose I ever shall go. You know, they write and say, well, we have now made you our vice-president. Why the hell all of a sudden have I got to be made vice-president? I mean, I don't even understand cricket or anything like that. I could have a grand social life, but we're not, we're not that sort of people. I never mind, or Andy, we never mind anybody coming here and we drink and things like that, but we're not people for going out a lot. We've always been home people."

Another winner also made the same point about the social *entré* that a pools win may provide: "I mean the win changed us. Look, they can all say what the hell they like but money goes a damn long way for making life . . . I mean, they talk about money's no this, or money's no that, or money's the next thing: but if you've got money you've got a hell of a lot that you wouldn't get if you didn't have it, like apart from what you buy. Money talks a long way for getting into different things, different kind of clubs and things like that. I've been asked to join things that I've never heard of before. I knew they were there but I was never qualified to get into that kind of company, and I refused an awful lot of these because I don't believe in this dress suit stuff. I've never worn a dress suit in my life and I've no intention of wearing one. (I was once kicked out of a hotel because I went in for a meal and I didna have a collar and tie on, so I was asked to leave). There's so many things up this way where you've got to dress up. The folk enjoy dressing up in bow ties and hiring suits.

It's up to them. But if the clothes that you're wearing are not good enough for the society that you're in, well to hell with that society, that's my way of looking at it, your clothes don't make you."

In the main, winners do seem to resist the invitations to join clubs and societies. Perhaps the following remark illustrates this point:

"Well I've never tried to be something that I'm not. I've never tried to push myself forward into higher class people. I've been introduced to some of them, I've met some people who are in the upper class, but I never – mind you, when I go away I stay in a decent hotel and I travel first class; that's about the only extra comfort I get."

This general reluctance to join clubs and societies, whether of a working class or middle class type, can be shown by the response we obtained to the question: "Are you a member of any social, recreational, religious or political organisation or club?" Fifty-one per cent of the comparison group replied "yes" to this question compared to forty-three per cent of the pools group. The tendency is confirmed if the figures are compared for pools winners before and after the win: forty-five per cent replied "yes" before the win, compared to forty-three per cent after. So in this area of life the pools winners claim that "it won't change us" seems justified at least.

What, however, of the pools winner's own view of his social position? To what extent do income and financial resources affect this view? If his subjectivity is conditioned by material factors, then he should as a consequence see himself as middle class. However, the transition is not as simple as that. Pools winners tend to be less inclined to ascribe themselves to any class position, as the Table which follows illustrates:

Do You Belong To A Social Class?

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Yes	25	28%	46	52%
No	56	63%	33	37%
Don't know	8	9%	8	9%
Non-coded	0	0%	2	2%
TOTAL	89	100%	89	100%

Fifty-two per cent of the comparison group thought they belonged to a social class, compared to only twenty-eight per cent of the pools group. Perhaps the remark which illuminates this finding most came from one winner in reply to the question on whether he felt at home in his neighbourhood: "Yes, because it's on the edge: middle class that way and working class the other." A similar kind of point was made by another winner in direct answer to the question: "I don't know what to anser to that one. I would have said working class at one time, but can't say that now." However, some working class winners are reluctant to give up their class identity, at least for some time after the win: "I should say I'm very rich working class at the moment." To which this winner's wife added: "Yeah, I still like to think of us as working class."

Thus the win does effect their view of their social position and so perhaps it is not surprising that other beliefs and values are altered too. The voting behaviour of the two groups suggests this most forcefully, as the Table which follows illustrates:

Voting Behaviour

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Conservative	52	75%	26	34%
Labour	8	12%	40	52%
Liberal	1	1%	8	10%
Communist	0	0%	0	0%
Nationalist	1	1%	0	0%
Other	2	3%	0	0%
Don't know	0	0%	0	0%
Refused	5	7%	3	4%
TOTAL	69	99%	81	100%

Twice as many pools winners compared to the matched group voted for the Conservatives. This is a novel illustration of the Marxist notion that political consciousness is determined by economic circumstances. Sometimes the switch is explained with quite elaborate logic:

"I am still a Labour man, and I have always been a Labour man. However, I can't afford to vote Labour any longer, I have to look after number one like everybody else, have to vote for the people who look after the money. But I'm still a Labour man myself." Another winner who laughed, when he was being interviewed, at the realisation that he had changed voting allegiances from Labour to Conservative, put a similar point, although more tentatively: "Well, I'm glad you change, you know, because, I suppose, because of my investments . . . that's the main reason, I suppose. I think, you know, Conservatives, they are better at running things financially see, and I don't think Labour is. We need a strong Labour group as opposition to help the workers, but whether they're any good at running things financially I wouldn't like to say. I started voting Conservative about five or six years after I had won the money."

This winner, a postman at the time of his win, spent much of his time studying his investments, which he enjoyed doing, as he had always "liked figures" (other winners too became interested in the financial and investment aspects of the win, although probably they are in a minority; we have no firm statistical data on this). One fairly elderly winner stated that if he had a big win again, "I should just try to do my own business . . . engage my own broker and buy what I want – shares – and invest it as I wished. I think I've learnt a *little* bit about – I'm not an expert on it – but I think I've learnt a little bit since about investing." He went on to say that he'd voted for both parties, looking back over his life: "Well, you see, I can be a capitalist, and I can be a trade unionist, remember I'm a bit of both, I can't chop myself in halves, so there you are."

And another winner, a committed socialist, became quite alarmed at his sudden wealth, because he feared he would become a Conservative overnight! He was relieved to find that he hadn't and, in view of the strength of his views, in his case it seems unlikely that he will do so, at least for some time. Not all winners become "turncoats", as one put it, and of course for the Conservative voter there is no problem of changing allegiance.

A further area were attitudes and beliefs are affected is religion, at least as reflected in the frequency of church attendance. Twenty-nine per cent of the pools group reported going to church once a month or more before their win, compared to only sixteen per cent after it. The figures for worshipping less than once a month were virtually stable: thirty-four per cent before the win and thirty-eight per cent after it. But lower church attendance was again confirmed by the number reporting that they never visited a place of worship: thirty-seven per cent before the win and forty-six per cent after it. The same tendency for Mammon to replace God can be seen in the results which contrast the pools group with the comparison group. Here, twenty-six per cent of the latter reported frequenting church at least once a month, compared to eighteen per cent of the pools group. From other evidence it would appear that the trend is probably

simply towards less frequent church attendance by those who, before their win, had been the most consistent worshippers.

How far, though, did winners in general feel that change was a consequence of the win? We attempted to obtain some estimate of this by including in the interview schedule the question: "Some newspaper reports suggest that some winners don't change their way of life very much as a result of their win; do you think this is the case?" Fifty-one per cent agreed with this statement, twenty-six per cent disagreed and nineteen per cent didn't know (the remainder were uncoded). Often a reply was based on personal experience: "We didn't change ours" – "I was happy as I was." Some denied the possibility of changing at all: "You can't alter people," or: "Some, like myself, are too set in their ways to change." With other replies, however, the emphasis is on the winners having no desire to change: "They are content as they are." Sometimes, external constraints are seen as making change difficult: "Well, socially you are not accepted about your station in life" – "Because of the publicity always chasing them and keeping the lime-light on them, it stops them changing. If they do not change, the papers lose interest."

The twenty-six per cent who thought that winners did change, gave variations of the "money goes to their heads theme," which was the most common explanation given. Other replies stressed the "positive" changes that may result: "It must change your life to some extent" (husband); "More security, more of the luxuries, better holidays" (wife). Sometimes, inevitable and extraneous reasons were emphasised whether "desirable" or not:

"You can't keep your same way of life, it's impossible. With a big win on the pools, with £100,000 on the pools you can't live in a council house, you couldn't work with chaps slogging their guts out for £20 per week. Well, you could, but you'd have to be a very heartless person and I don't think it's possible."

A few replies also stressed the pressures from other people to lead winners to change: "They are too easily swayed," or: "People are awfully weak, they can be pushed any way."

Others suggested "snobbery" was a cause: "Because they've got money they become snobs; I've remained working class," or: "In my opinion it always changes them. It changes a lot of peoples lives before they know how much they've won: some had eight draws up on Saturday and didn't want to know you on the Sunday morning!"
The "don't know" replies often stressed that it "depended entirely on who won," or: "Different people react in different ways."

Generally, when the changes are regarded as undesirable, they are usually ascribed to others rather than the particular respondent himself; others "let it go to their heads," or become "snobs." And although it may be self-deception that such unfortunate changes do not occur to themselves, an awareness that they happen at all, may have some effect in minimizing the adverse consequences that could follow from a big pools win.

The wording of this question on change might have encouraged respondents to have agreed with it and to that extent, underestimated the degree of change. But there may well be other factors at work too. The "stigma" aspects of being a pools winner (which have been discussed earlier) might also contribute; it is possible pools winners dissociate themselves from the unfavourable aspects of their public image by emphasizing that they are exactly the same as they were before the win. Furthermore, if winning the pools is a threat to the identity of the individual concerned (as was suggested at the beginning of this chapter), then that threat can continue to be minimised by denying the possibility of change. Finally, the changes that do occur, probably do so over a period of time, and therefore, in many cases the winners may be hardly aware of them at all.

However, the whole question of the essential nature of an individual's "self" is very complex and quite outside the scope of this book; such changes that we have discussed (for example, political and religious attitudes) may not, in the eyes of the winners, constitute a very basic part of their own characters and can therefore be discounted by them. A comment from one member of the sample suggested this,

when he remarked that he didn't take politics too seriously (he had remained a Labour voter). And if this is the case, it is not surprising that, the wife of one winner, when she was asked if she and her husband had changed, replied:
"Not really; only we have a nicer house and a nicer garden and that, otherwise there's not much difference, is there?"

There is one tendency which may occur occasionally, although this is at best only an impression for which we have no statistical support, and that is "amplification effect", which the win has on the pre-existing characteristics of a winner's personality, exaggerating or amplifying features already present in his individual nature. For example, one winner described how he had always been a practical joker, but since his win, his opportunities for such escapades had considerably increased. On one occasion, when he had had a disagreement with someone, he ordered a dead horse to be delivered to their front lawn late one evening, so that they would discover it when they woke up the next day! It should be added that he arranged for the local council to come and collect it. Similarly, another winner, who had always been shy and retiring, found that after the win, with no need to work, he met fewer people than ever, though this did not, he claims, trouble him. But even for someone as quiet as he was, the pools win had brought some new experiences:

"When I first won the money, you know, I bought a car and I passed the driving test and I used to go all around Warwickshire just exploring in the car, but I don't these days."

Thus, the evidence from our research is that the lives of those who win on the pools are changed over several, though not all, of the important areas we looked at. Not only the "external" aspects of life-style based on material wealth are affected – being able to give up work, buy property and take more holidays – but also the more personal spheres of political and religious behaviour. Insofar as winners sometimes make this distinction between their way of life and their own characters, they probably underestimate the extent to which their personalities have been shaped by the win, however "successfully" they may have adapted to their new

circumstances. Nor should this be surprising since personality is, in part at least, a social construct formed from an individual's life experiences.

Yet there is a sense in which the pools winners are still right when they say that they haven't changed: very few have undergone the radical transformation that they probably feared at the beginning of the experience. Winning the pools alters people less than, say, going to university sometimes does. The political allegiances of the winners may have moved from left to right in many cases, but they do not adopt political values outside the conventional range. Similarly, their life-styles tend to be comfortable and conformist, rather than idiosyncratic or adventurous. One described his experience as an "apotheosis", but even his "transformation" was very much within a conventional or conservative norm. In the main, a "nice" house, a "nice" car and a "nice" garden are ways in which the win is used. This conclusion, however, makes no allowance for the problems and difficulties which confront many winners in their attempts to come to terms with their new wealth, and these questions are dealt with in the next chapter.

CHAPTER XI

Problems And Happiness

"Listen, if you ever went to London, it's worth it; there was a room in Grosvenor House which Littlewoods take – it's an agreement – there's a set of rooms reserved for Littlewoods, they hold their board meetings there, and we had our meals in that boardroom. There'd be some of Littlewoods directors and some of their men who worked for them, and the meal was marvellous. There was a table across the corner, cigarettes – oh, piles of twenty like this, of all makes – bottles of everything, everything . . . If you went out after breakfast and one of those bottles – a half a bottle of rum gone perhaps – it was replaced by another one by the time we came in for lunch. And at the first night that we were there, I think there'd be 150 to 200 reporters and photographers, and everything was free: double gins and double whiskies. They had a real do, and that was from tea-time until about one or two in the morning. And we had a lovely suite of rooms, my wife and I, and our four girls, they had the rooms that the Littlewoods directors use near the boardroom; they took a suite off every time and they use it, just reserved for them . . . ."

For many pools winners, the reception in London is an exciting and pleasurable occasion; for some the smoking, drinking, shows and diners during the four days of the reception had been "the best part of the whole win". The experience of becoming a pools "star", the exhilaration of being the centre of attention and enjoying such totally novel luxuries, soon wears off of course, and the host of problems which comes with publicity swamp, for many people, the pleasure and happiness of the time in London. Years after the

win, however, some winners look back to this time with great nostalgia, but because of the problems which follow, are glad of their ability to fade out of pool stardom into obscurity:

"It was a nice feeling at first to be noticed for a change you know, but the glamour does wear off. But to have something like that happen to you every six or twelve years, I think that would be nice. I wouldn't want to be in the limelight like the Queen and stars and anything like that; you can bow out quickly and come out; they can forget you then."

Referring to this early period, a number of winners mention being incorrectly quoted in newspaper stories and being made to look foolish and ridiculous in television and radio interviews, and some of them harbour considerable bitterness about this many years after it happened. One man interviewed on television during the reception period complained afterwards that "they made me look a right mug and it was a load of old cobblers". This feeling about being misrepresented by the media persists as a problem for a number of winners many years after the win:

"They came and we talked to them, and there was an article in the press as if it was a rags to riches story. It was a packet of lies from start to finish. All we could do was to challenge it and make it worse: it was like a steam-roller and started it all up again."

One winner complained that they "made him feel like an idiot"; another felt that he had been deliberately deceived by one of the national dailies, which held a party for a group of winners – promising no publicity – and then prominently displayed their photograph on the front page of the next day's edition of the paper. One of our respondents claimed that they had attempted to correct the false impression of earlier stories by giving further interviews, but found in practice this simply didn't seem to work: "I used to give interviews because I thought that I could make them see it my way, but they always have their story written before they come." In fairness to the media, it should be pointed out that of forty-two winners who said they had been approached for stories about their life after the win, only six spontaneously

mentioned that reports of interviews had been misleading. But as we did not specifically ask a question on this, it must be a minimum figure.

Almost without exception, however, the pools companies themselves were seen as being helpful, friendly and approachable – "they couldn't have been better really" – and this positive image starts with the "wonderful experience" of the reception: "The whole atmosphere at the Grosvenor House, it was smashing, weren't it? They were all friendly, they all spoke to you; no snobbishness about it, was there?" This initial perception is later reinforced by the way the pools companies through their advisory services help the winners in dealing with the various practical problems which inevitably arise. As a result, the companies are often thought of as being "really magnificent people". Some pools winners are in a highly vulnerable and anxious state immediately after the win, wondering what they are going to do with their money, whether to give up their job, how much to give to various relatives and so on. Advice from the pools companies is usually welcomed as a means of solving these problems, sometimes to an extreme degree:

"Oh yes, he's just like a father to us . . . the pools company doesn't want us to worry about anything, so they're quite prepared to take anything on. Any trouble at all, and they'll sort it out for us."

The service that the pools companies provide even includes help for winners abroad: "You feel lowered because people follow you around; we get people following us in the street, pointing us out. When we went to Africa, it was only the pools man intervening that stopped the local paper from reporting that pools winners were in the district."

The pools representatives who call in the years after the win sometimes become very friendly with winners and the positive attitude towards the pools companies partly comes from a feeling that they are genuinely trying to help: "Really they could dump it in your lap, and that's it, and let you get on with it, but no they don't, they're grand people."

A more complex but perhaps ambivalent understanding of the motivation of the companies was expressed by one of our

respondents: "I think they're genuinely concerned really, because I suppose they could get bad publicity if anything went wrong." All pools companies are in a difficult situation as far as publicity is concerned: on the one hand they dread the bad publicity from the winners who "go off the rails", while on the other, they need to publicise really large wins and the people who make them, in order to boost and sustain the numbers of people who do the pools. A number of pools winners report that the companies put a great deal of pressure on them to agree to publicity: "Littlewoods explained to me that they make their living from publicity when the win is over a certain amount" – "Well I was the first big winner that year and Vernons were desperately looking for winners to get the pools started up again." Normally, many people might resist this pressure to publicise the details of their win; after all, about nine out of ten people who win the pools put a cross for anonymity on their coupon but at the very most, one in seven actually manage to retain their anonymity after winning. It is not difficult to see why the companies are so successful in persuading people to change their minds; the sheer exhilaration and happiness at winning such large sums – "We were doing plenty of laughing, it was the climax of the few days after we'd won the pools, when we'd actually got the cheque" – predisposes winners so favourably towards the pools companies, as to overwhelm all but the very strongest resistance. As the brother-in-law of one winner put it, when asked by the press about publicity: "for £50,000 you can spread us round the world." Many winners have a more general sense of appreciation of the pools companies activities on their behalf: "The pools companies were being very good to me, so the least I could do was try to show them I appreciated it also."

There are other arguments that the pools companies can use: "People will ask where the money came from" – "Isn't it better to get the thing over with as soon as possible?" The anonymous winner who Vivian Nicholson mentions "found it was hard to keep" his win "secret because he couldn't tell people what he actually *did* for a living." Certainly some winners believe that it is inevitable that their identity will be

discovered and they might as well get "a bloody good time out of" the pools company while they can. One or two respondents claimed that their identity had been revealed through having to make a public claim on their winning coupon (under pools companies rules, a person's win can be disallowed unless such a claim is made): one originally anonymous winner felt that the post office official who processed his telegram had tipped off the press about his identity. Another of our respondents thought that individual representatives of pools companies were the source of information about winners movements and whereabouts: "All the bloody pools company people leak it. I mean the pools company themselves don't do it, but they've got people that take backhanders, I'm sure they do, a fiver off the press and they let them know how things are going on." In one or two cases, these leaks are seen as having been deliberate acts on the part of the pools companies: "the X on the coupon was worthless; we think the pools company paid someone at the *Express* to leak our name." Of course, there is no way of proving these allegations, as the potential source of leakage is very great. Sometimes it is the sheer weight of the numbers of people who call from the companies that is a decisive factor; one Scottish winner reported that "ten men, and nine of them Scots" called at his house, some of them pools representatives, some of them photographers: "You knew perfectly well they would be behind the vestry the next day, whether you gave the firm leave to publicise or not. You knew they would go about shelling out lies . . . and you couldn't control them, and that was the reason I did give them publicity." However, it should be stressed that a small minority of all winners do remain anonymous, and they appear to achieve this by refusing all forms of publicity whatsoever. The pools companies sometimes put psychological pressure on winners to attend, by witholding information about the actual size of the win until the presentation ceremony at the reception; but of course winners can and do refuse to attend this.

There are good reasons for retaining anonymity; in some respects the effects of publicity create more problems for

winners than any other source of difficulty. As the accounts in the first part of the book illustrate, the most immediate problem is the deluge of begging letters that winners receive, although the pools companies will sort and destroy letters as a part of their services. They also usually warn winners of the activities of "professional" letter writers (the letter ostensibly from parents asking Richard Taylor for money "to bring their son's body back from abroad" was in this category). Some letters appear to be genuinely sad, although most of them are probably fake. It is in the interests of the pools company to emphasize this aspect of the letters, as it helps to discourage winners from spending their money, and therefore minimizes the chance of them losing it all and creating bad publicity. A number of letters, although intended to be serious, do have their more comic aspects. One woman "living in a council house", wanted a winner "to buy her a farm in Devon so that she could let some deserving couple have her council house". Another letter which gave the recipient a certain amount of pleasure, appears to have come from two religious ladies living in Walthamstow: "Dear Mr. Finch, you've won £75,000. The Devil will get you and you'll go to hell. We will pray for you." However, the overall effect of begging letters and personal approaches is anything but amusing. One 71-year-old spinster, who won about £100,000 was so bombarded during the first month after her win that she was reported in the press as thinking of leaving the country: "I'm beginning to wish I had never won the money. I am fed up with all the begging letters, the proposals and all the friends I have suddenly found. All I want is a bit of peace and quiet and the only way I shall get it is to leave the country."

Although begging letters may cause distress, they can be destroyed without having been read and they usually stop within a few weeks. They are much less of a problem for most winners than personal approaches from people either calling at the house or stopping winners in the street; as one winner put it: "All the time it's knock, knock, knock on the door". One middle-aged couple had been very frightened by a man who "hung around till nearly midnight" demanding

£2,000 to start a business, and a similar incident with another winner led to violence:

"I struck about six folks. I knocked one out through the door of the house. He'd been at the house two or three times, and I hadn't been in. I went to the door this night with my son. We had an outside door and an inside door; he'd opened the outside door to come in and I was half-way through the inside door. 'Well look', I says, 'bugger off, I'm no having nothing at all to do with you'. 'Oh but . . . ' and he started, he says, 'it's no bloody use you getting high and mighty with me,' he says, 'I ken you're a working man'. And I lost my ire and I belted him."

Some winners find that the approaches from people for money drive them to a point of extreme anxiety: according to a newspaper report, five months after the win one winner's wife "was so badgered by people after money that she had a nervous breakdown. She went into a hospital, where a doctor warned her she could not survive the continual siege of salesmen, borrowers and beggars." The couple's problems were only solved when they moved out of the area, where they had lived for thirty years, into a neighbourhood where they weren't so well known.

Being followed in the street is less personally threatening but can also lead to a sense of being persecuted; one winner said that being stared at in the street made him feel like a "film star or a freak" and Vivian Nicholson's phrase "pools freak" refers to the same feeling. Winners find quite trivial actions being reported in the local and sometimes even in the national press. One man who committed a minor motoring offence about a year after his win, when asked what his occupation was, had said that he was "unemployed"; the press picked this up and claimed that he had been treated with extra leniency as a result of this, although the winner denied that this was in any way intentional (many winners have difficulty in describing their employment status: if they are not working and they are fairly young, they are reluctant to call themselves "retired" which really only leaves them the category "unemployed"). Some winners become so famous that their houses become places of interest on bus

tours: "Buses of tourists were told that is the place of the man who won the pools". Others find that years after the win they are still recognized and stopped in the street; one winner's wife was stopped by a local journalist outside Marks and Spencers and asked whether she still did her shopping there. There appears to be widespread curiosity about how pools winners spend their money and many people seem to have strong feelings about how they would spend the money if they were in the winner's place. This leads to further sources of friction, such as the occasion when one winner was having his hair cut:

"I went to the local barber and he and I were just talking, it was three or four days after I got the money. I was sitting in the chair getting my hair cut and talking, and this lady and gentleman came into the shop. She says 'Och, I didn't think the like of you would come in here. Why don't you go get the barbers to come to your house.'"

The winner was particularly upset by this, as the barber was a long-standing friend, and the idea that he should change his usual way of having his hair cut because of the money, seemed to him rather shocking.

Not all winners, of course, suffer from this kind of problem, and the variations in other people's responses can be seen in the eight stories in the first part of the book. Many winners find that "after the first few weeks" the publicity dies down and they can more or less get on with their ordinary lives without people constantly stopping them and staring at them. As one of our respondents put it: "Some people look at it as a ten-day wonder; they think you've done something marvellous, but it fades into the background and things are normal now." Another winner had the same kind of experience – "I felt my old neighbours were afraid to speak to me, but I call and see them regularly now and that feeling has worn off", as did a third: "Very close friends were afraid of seeing me at first for fear of seeming to be begging; they kept away for a short while, but soon everything was O.K. when they got used to it."

Some find that their fame is not so easily shaken off. One couple had had to move house four times in order to find

anonymity, but were still being recognised as pools winners, which they thought was partly due to the fact that they had two red-haired daughters. Several others of our respondents suffered from the same problem: "One of our old neighbours said someone who'd moved into the street was asking about us and our being pools winners. Sometimes when we are out we still see people pointing at us and we know they are discussing the win." Whether a person stays in the same area as that in which they were living at the time of the win, obviously has a lot of influence on the likelihood of their being recognised; also the kind of community – whether it is a close-knit working class community or not – probably has some effect.

The fame that can come with winning the pools brings special problems of its own: one or two winners mentioned the fear they had that their children might be kidnapped: "You worry about if anyone will try to take the boy off, and kidnap him and all sorts like that for ransom." Similarly, there have been burglaries committed at pools winners houses, which might have been due to them being known to own substantial sums of money; one such incident occurred when "three men dressed in pin-striped suits and bowler hats had tied up a winner's wife and au-pair girl and had burgled the house." Much more frequent a problem, however, is the difficulties that winners have with people expecting money, gifts and subsidies in various forms: "People expect you to back them in their businesses and give them financial assistance and if you don't give, it's like as if you weren't friends somehow." One winner who was running a butcher's shop in a working class district found that the publicity from the win and its aftermath forced him to give up the business which he had owned for thirty years. On hearing of his win, he was reported in the press as promising to buy each of his regular customers a free joint of meat; on his return from the reception in London he found a large crowd outside his shop, which had queued for over three hours for its supply of meat. A fortnight later he was interviewed by a local journalist who reported that housewives "expect him to give them the best joints for next to nothing," and that custo-

mers mention "how poor they are the moment they walk into the shop." A number of winners who have set up businesses of their own have found that customers expected goods cheaper than they would normally have had to pay for them. One man started a greengrocery shop and was interviewed by the press soon after it was opened:

"Customers are likely to ask me to knock a penny off a pound of potatoes because they know how much I'm worth. They used to try it at Covent Garden, you know. Another chap at the market pays fourteen bob for a bag of sprouts, but I get charged sixteen shillings because I'm a pools winner."

Winners also get charged more than they should have to pay; for example, a tailor tried to charge one winner more for the suits that he had made for him than "was usual". Another of our respondents found that the fishing club of which he was a member expected him to buy them a new club-house – which he refused to do – and this led to him not being re-elected Chairman of the club, an honour which would normally have been his.

Winning large sums of money can also attract people with goods and services which are difficult to sell:

"I've been offered furniture, I've been offered central heating, I was offered four colour televisions, £100 for four. There's an awful lot of people seem to think because we've got money and we're working folk that we're ready to buy "hot" stuff. About the strangest thing that I was offered was a wagonload of bogey brackets, you know, on the railway, where the rail sits on the sleeper, they call these bogey brackets. I didn'a have a clue what he wanted me to do with them! There were so many different things: radios and cigarettes, there was a lot of cigarettes. Well a man came here and offered me two daughters: he was in debt, something like that. You wouldn't credit how they can behave."

Money can also lead to mild forms of corruption amongst friends: one winner had been "approached" by a journalist who had been given his address by a friend for two pounds, which had been a great disappointment." This kind

of experience leads to people becoming harder and in some ways perhaps more selfish:

"When we won the money it started to create an impression on me that people were trying to get something for nothing; so I withdrew into meself a bit you know. I was sort of thinking to meself, 'Well this has got to last me a lifetime, I'm going to hang on to it.' To a certain degree I became a bit selfish if you like, which I certainly wasn't before. If anybody asked me to lend them a fiver, instead of saying like I would have done before, if I'd got the fiver, yes, straight away, I stood back and weighed it up, why they wanted it: did they want it because they knew I'd got a lot of money and they wouldn't be expected to pay it back? That sort of thing you know."

Other people's attitudes to pools winners in some cases has an immediate impact on their life circumstances. Several winners living in council houses were forced to move: "The council said we had to get out of the council house because of the win", while others found the reactions of neighbours had a similar effect. One couple living in a council block of flats had found that the working class people in the area became very hostile, which included one of the neighbours children dropping "a paving stone on top of the car from four floors up." The couple added in explanation that they had taken "some neighbours out and given them a nice evening, but couldn't do this for everybody." Occasionally, pools winners are forced to give up their work against their own inclinations; a wife stated that her husband had given up work as a shoe-edge-setter "because people were on at him; it nearly broke his heart to give up work, but workmates said that he was taking a job away from somebody else; he lived for his work, he loved it and had no hobbies; if he could have carried on, it would have given something to occupy him." Although this is a fairly extreme example, there are a number of winners who told us that this feeling that they would be "taking away" a job from somebody else was widespread amongst their workmates.

Winners reported few feelings of guilt on their own part, or moral disapproval from others about winning such large sums

of money. One confessed that the reason why he hadn't wanted to go out and meet his friends was because he felt "a certain amount of guilt" about getting "something for nothing;" another "had a sister and brother-in-law who were very religious, and they thought it was not right," whereas a third couple who had strong religious views, were sufficiently worried about their church's disapproval of gambling to try to remain anonymous. Presumably, doing the pools is now so widespread that there is little or no moral objection to those few people who receive large wins. Occasionally, as in the case of David Llewellyn's story, outsiders do object to such large amounts of wealth, or as in Richard Taylor's case, reproach him with never having "earned a bloody penny in his life," but most pools winners do not seem to have too much difficulty in rationalizing their situation. John Sellens felt that winning the money "was probably some repayment for what I've done" in helping people; another man who won over a quarter of a million pounds argued in justification for such large wins: "With the country in such a bad way, the small slender chance of winning the pools gives people something to hope for, and injects a bit of colour into many drab lives."

One of the most depressing problems that faces many pools winners is the jealousy and envy some relatives and friends feel: "My husband's mother was jealous; we couldn't go out and buy anything because she got jealous. Every time we bought, we were buying it for her as well." Frequently, this takes the form of relatives objecting to how much money they have been given: "Some of my relatives were jealous. My brother hasn't spoken to me since, although I gave his son £800." And similarly, according to one winner's wife, "his brother called us skin-flint; I thought, my golly, if somebody gave me five hundred pounds I'd get down and kiss their feet I think." Often these feelings of jealousy are based on invidious comparisons, leading to permanent breaches of family relationships:

"Peter's mum used to think that my mum was getting more than she used to get, and they had a big argument didn't they? They used to get on quite well before, they used

to go to the same bingo place and they always used to share what they won on the tables and that's all stopped now. They fight like cat and dog now, and yet they never used to; they used to get on ever so well together."

Winning large sums of money can also completely disrupt the existing equilibrium of family relationships, so that the status of one member is completely overturned compared to another. An example of this was given by a winner when talking about his relationship with his brother:

"We were sort of in the same state in our lives, we'd got the same ambitions and the same plans, we'd just both purchased our house, hadn't we? The basic fact was that he became envious and jealous because I'd suddenly overtaken him in a great flash and a cloud of smoke, you know. And I was miles in front of him, kind of thing; we were sort of competitive before, you know. And then all of a sudden he was stranded . . ."

These problems with relatives are sometimes overcome with time; as one winner put it: "Our relatives were envious; they expected us to give away half our win and there have been difficulties; but we've got over them."

There is obviously more pressure on winners to resolve difficulties with their relatives than their friends; problems with friends often lead to a complete break with them: "My friends did not react very well. The majority of them became embarrassed: before we used to go out, everyone had the same sort of money in their pocket; so now I really don't have the same friends. My friends then were in the £1,200 – £1,500 bracket; now they are in the £3,000-4,000 one." Friends and acquaintances also react in the other ways in which we have seen relatives react: one winner gave money to a number of his old workmates, including £30 to a person in difficult circumstances; when told of this, this particular person asked, "Is that all I'm getting?" Another winner found that friends were jealous of her and her husband on account of their youth, she being twenty-seven and he thirty at the time of the win. Vivian Nicholson found the same thing from her old acquaintances: "Why them? They're so bloody young, we've been filling in the pools for years." A

number of our respondents came to feel that some of their friends before the win weren't "real" friends after all: "You just couldn't tell who were real friends and who only wanted to know you for what they could get out of you."

Although many people appear to be genuinely pleased that relatives and friends have won the pools, there is a certain amount of indirect hostility which appears to be based on envy. Perhaps an example of this was when a girl approached the nine-year-old daughter of one pools winner at school and told her that "her father had stolen the money," leading the daughter to "burst into tears immediately." On another occasion, somebody talking about a winner's wife's sister, who had just died after the win, said in the wife's hearing, "Oh well, for all her money, it didn't do her much good, did it?" One winner said she felt that people in general were just waiting "to see how much you slip up." A number of our respondents said they felt that whatever they did they would be criticised for it: "If you went into a pub and bought everyone a drink you were big-headed, but if you didn't buy them a drink you were a mean bugger." (This was the same as David Llewellyn's experience: "Someone behind me said in tones none too quiet, 'Well look at that mean so-and-so there taking a drink off that chap, when he could buy us all a bloody round here.") Similarly, on the way in which they spend their money: "If you get something of the best they'll say, 'I should think so – he can afford it if you don't get something of the best, 'Well fancy – all that money and he bought so and so, he hasn't got this and he hasn't got that!'"

It is possible to quantify the degree of friendliness or hostility shown by various sorts of people towards winners. We asked a series of "open-ended" questions on the way people had reacted to the win, and classified the answers in terms of whether they were positive, neutral, negative or mixed. Positive answers were the ones which said that people had been happy, pleased, good about the win and so on; neutral ones were ones which stated that people were the same, that there hadn't been any change in their attitude, that they were alright and behaved normally. Negative answers were ones where winners said that people had been

jealous, envious, resentful, etc.; mixed answers were a combination of positive and negative ones. Obviously this classification is a bit arbitrary and perhaps the statistics it yields have a tendency to understate negative, hostile reactions, as some of the worst experiences that pools winners have with people they know, appear to fade in their memories with time, and occasionally winners failed to mention negative reactions in direct answers to the questions, but referred to them elsewhere. The following table summarises the results of the classification:

People's Reactions To The Win

	Positive	*Neutral*	*Mixed*	*Negative*	*Total*
People in general	24%	34%	26%	16%	100%
Relatives	47%	34%	11%	8%	100%
Friends	39%	44%	12%	6%	101%

There is a fairly strong tendency for winners to report that people in general were more hostile than their relatives and friends, although even people in general reacted overall positively or neutrally (fifty-eight per cent) rather than negatively or in a mixed fashion (forty-two per cent). Most winners seem to have found their friends or relatives either pleased or happy about their win or at least more or less neutral about it. There was, however, a significant minority of all categories of people who reacted negatively in one form or another, not just the forty-two per cent of people in general, but also nineteen per cent of relatives and eighteen per cent of friends. Also, our respondents did occasionally indicate that they thought that the friendly reaction of their relatives and friends might be because they were "hoping for a couple of quid." One or two winners stated this even more categorically: "I suppose our close friends were pleased because they thought they was going to get something." Some winners were prepared to put it a little more gently: "Our relatives were very pleased for us: of course it helped them too." However, that money is not the overwhelmingly determining factor in people's attitudes is indicated by the fact that friends were felt to be as friendly as relatives about

the win, in spite of relatives receiving much more money than friends.

One of the more unpleasant features of the envious, hostile reactions is the tendency to circulate invidious rumours about pools winners. Although we did not systematically collect information about this, the rumour we most often encountered in our research, was that a particular pools winner has finished up in a mental asylum . But as far as we know, with the exception of Vivian Nicholson's voluntary admission for two days to escape from her second husband, in no case has this turned out to be true. These rumours can cause a lot of distress to their victims: a number of rumours were circulated about a particular couple, including one to the effect that the strain had sent the winner into a mental hospital. The family were sufficiently distressed to repudiate "this in the local paper in no uncertain terms." It was rumoured about another winner "that he had crashed his new car into a bus, had spent all his money and gone mad, and had been taken into an asylum." Because people also need to believe that if *they* won the pools they would be very happy, there is often an ambivalence of attitude towards pools winners: they are thoroughly miserable (envy) or they are blissfully happy (self-identification).

Many relatives and friends are worried that their friendliness will be misinterpreted as being after money: "Some say, I can't be friends with you, you'll think I'm after your money." A number of respondents – and this comes out in the stories in the earlier part of the book – state that their "real" friends felt like this, staying away so as not to be thought of as scroungers, whereas they believe that their superficial friends had reacted in the opposite way. Often this is only a short-term problem, with winners regaining old friends and relationships along the same lines as before the win. Long-standing relationships do get broken but on more fundamental grounds; one couple found that old friends of theirs -

"Were exceedingly nice at first; we gave them £1,500 towards a bungalow and they moved near us. They sold the damn thing and put the money into the bank and flitted back

to Bournemouth! We never see them now." The couple gave a possible explanation of this behaviour in another context: talking about their relatives they said that "they either feel uncomfortable when they come or they feel they can't keep up with us."

One surprising finding was the relatively little amount of snobbery that pools winners encountered; we anticipated that they would experience a certain amount of social rejection on the grounds of them being "nouveau riche." One of the reasons for its absence is that very often other people don't know about the pools win, and this is particularly the case where a winner has remained anonymous by moving to a new area. It was for this reason that one pools winner was so anxious to conceal his identity; he said that "people look down on you if they know that previously you didn't have much money." Another is the tendency of British people to "keep themselves to themselves": a number of winners mentioned that they had little or nothing to do with their neighbours and reported this as the prevailing social pattern in their area. This makes it easier for people who have acquired wealth overnight to be absorbed into a new community with a social status higher than the one they have come from. One working class winner who was very anxious about being accepted in his new middle class milieu, found the solution to his problem when he moved onto a new estate: he was one of the first people to move into the area and "they were already settled" when their new neighbours – architects, doctors and solicitors – began to arrive. The vast majority of our respondents felt that their new neighbours accepted them: ninety-one per cent said that they found people in the area friendly towards them, the same percentage as that in the group of people in the matched comparison group. We have seen in an earlier chapter, however, that winners have significantly less social contact with neighbours than do members of the comparison group. Whatever the reasons for this, it does appear that winners feel just as much at home in the area where they are living as their peers: thirty-eighty per cent of them said they would be very sorry if they had to leave the area, as against thirty-two per

cent of the comparison group. There are, however, some clear examples of snobbery on the part of pools winners' neighbours:

"The people like where I'm staying now, they're a different class of people from the people that's in the tenements or the council houses. I would say, maybe three out of ten, maybe more, of the people round here say to me, 'What does a person like you do with the money?' The people that are of this class, these are our teachers, doctors and things round here, people that's in the professions . . . Three months ago a lady up the street asked me, 'What could the like of you do with all that money' . . . And there's a gentleman across here, he's a company director, and the first thing he said after he came here was, 'So help me God, I didn't think I'd have to stay aside the labourers."

Although this is something of an extreme case, there are certainly other examples of such snobbery. One couple found that this came out when they were away on a cruise:

"Somebody did find out, and because you've got your money this way, people began to think that you were an absolute peasant before, that you didn't know how to use the right knife and fork, you know, that sort of person."

Most winners, of course, manage to disguise their identity in this situation; at least this is the impression gained from the tape-recorded interviews. As with most of these examples, however, it is possible to find an opposite one to counter it: John Sellens in his story describes how easily he mixed with people from a very different background on a number of cruises probably the most enjoyable part of his win! Another winner describes how he was treated as a V.I.P. when it was discovered he was a pools winner: he was on a world cruise, and his boat was docked in Durban when a local journalist's wife found out who he was ; she then arranged a reunion with somebody living in Durban who the winner had met on a previous world cruise.

One aspect of the snobbish reaction towards pools winners is the accusation that they fecklessly squander their money, spending it on "vulgar" consumer goods, or lose it through

their own gullibility and incompetence. This myth is to some extent fed by the media's interest in those winners who "go off the rails," and who obviously provide more sensational copy than the majority who don't. Of 144 winners in the main part of our study, only thirteen appear to have lost more than three-quarters of their money by the time we had interviewed them; this represents only about nine per cent of the total (one in eleven) and even this minority can hardly be said to fit the characteristics of the mythical young working class pools "loser." The average age of these thirteen winners was forty-seven, a year older than the average – forty-six – for all of the 144 winners. Even on class background, the thirteen people who had lost most of their money do not bear out the stereotypes: twenty-three per cent of them were middle class, compared to thirty-one per cent of the total group; indicating that the class composition of the two groups is not all that different. Nor does it seem to make much difference from which company they received their win: there is a very slight tendency for the thirteen "losers" to have won their money with Littlewoods, but this is not statistically significant. This finding suggests that Littlewoods' attempt to prevent winners from losing all their money by setting up their advisory service has had little effect. It is true that there are proportionately more winners who have lost most of their money who received wins before 1957, when the advisory service was set up, than afterwards: sixty-two per cent of the "losers" won their money before 1957, as against forty-three per cent of all winners, but this might be due to the obvious fact that the longer you have had your money, the more chance you would have to lose it (this question will receive further attention in our next, more analytical and theoretical book). It is sometimes argued that many people are incapable of handling the very large sums of money given out in pools wins; the small amount of evidence which we have on this suggests otherwise. The average size of win amongst the thirteen "losers" was £90,000, whereas that amongst all winners was approximately £115,000; suggesting that those who made the *smaller* wins were more likely to lose their money. However, this makes no allowance for

changing values of money and, given that the "loser" group received their wins significantly earlier than the all-winner group, there is probably very little difference in the average real values of the comparative wins.

We have referred to the thirteen winners who have lost more than three-quarters of their win as a "loser" group; in some respects this term is a little misleading, as it covers a range of different ways of becoming moneyless. There are basically four such ways: giving the money away, losing it through being defrauded, investing unwisely in business, and spending it. In practice these overlap in any one case, although there is a distinct tendency for each case to fall in one category rather than another. However, the categories themselves often blur into each other, as the following experience that one couple went through shows:

"Brother-in-law asked us for £1,900 to put down a deposit for getting a house built. We gave him the cheque, but the builder came and said he hadn't received a penny and asked us whether we could pay him. We then found out that brother-in-law was using the money to keep himself out of jail: he had stolen that amount from his employers and was paying it back."

This incident involved both giving money away and being defrauded. Another example of this is to be found in the rather sad experiences of one winner; with his money he bought three boarding houses in Blackpool, one for himself and one each for his brother and two sisters. He and his wife found that running a boarding house meant "humbling themselves to people," so they sold up and acquired a small two-acre market garden. According to a press story, during this same period "he was full of generosity, and dished out cash right and left – anybody could have a bit." Friends who got to know him well before he emigrated to New Zealand said that he had been "taken down" by various people: "A man came to the door and 'sold' the winner a car that never arrived", and this kind of thing happened frequently. The winner himself was quoted in the newspaper story as saying, "There are people watching out for people like me, and they took thousands off me." Eventually the winner reached a

point of bankruptcy, having to sell his flower nursery "with only about £200 in his pocket left." He turned at this point in time to his brother and sisters for help, they still owning the boarding houses in which he had set them up, but they refused to help. According to one of his friends he felt very bitter about people in general and his relatives in particular: "He said that if he had a gun he would shoot his brother – and I think he meant it" . . . "he also said that he would like to put his relatives on a boat and shoot the bloody lot." Obviously, this kind of indirect evidence about the winner's feelings should be treated with caution – it is possible that his own account of his feelings about the win might be very different if interviewed today.

Some winners are particularly vulnerable during the early period after the win when they are feeling a general sense of elation or are so "intoxicated" in the aftermath as to have their judgement seriously impaired. One man was accused of a breach of contract over the purchase of a £20,000 hotel in 1952 but according to press reports of the court case, successfully pleaded in his defence that he had been drunk at the time of signing the contract. Another winner's vulnerability was his very poor judgement of honesty and credibility of business associates: amongst other business ventures he became involved in the "Speedway" – he invested money and "kept putting it in and putting it in to keep it going and then the people frauded him out of it." This was one of several business failures and, together with a number of loans which were never repaid and acts of personal generosity, was enough completely to wipe out all his money and force him to return to work as a clerk. Quite a few winners refer to having lent money to people and never having it repaid; such loans are seen by the beneficiaries as virtual gifts, on the assumption that winners with their large amounts of wealth would never have the "moral right" to ask for it back. Several of our respondents indicated that they knew that this would happen and therefore refused to give loans because of this: "People come and say, 'Could I have £100 for a week or two'. You know you won't get it back, so we refuse. And if they've been friendly before, they get a bit miserable." One

couple who did lend money out certainly had expected to get it back; when this didn't happen they took the people concerned to court. Five years later they were still pursuing the case and as they themselves said about it: "We wouldn't let go." Most winners, however, seem to resign themselves to never getting money back which they lent out to people and which was not spontaneously repaid.

Of course, as we have seen, the pools companies themselves give winners very careful advice on investment and disposal of money, the latter including how much to give to friends and relatives, as well as advice on business ventures. The companies suffer so much from the bad publicity of a winner who loses all of his or her money, that they go even further than giving advice: they keep a check on the state of winners' bank accounts:

"They keep in touch with the bank, I think, and when we started to draw a lot out, they wanted to know what we were doing with it, you know, and not throwing it away or anything like that."

This "looking after you" lasts in most cases for just two or three years, but in the case of "problem" winners the pools companies, particularly Littlewoods, appear to keep much closer contact and observation over a very much longer period of time. (In Vivian Nicholson's case, the company's representative, on occasions, even used to accompany her first husband to the bank in order to help sort out their financial problems.) The main difficulty from the pools companies point of view is that people do not have to take their advice: "Advice was available, but declined" – "I did what I felt best" – "The pools company wanted to advise him but he was a man, he wouldn't be advised." In some instances this can turn out disastrously for winners; one man turned down the advice that Littlewoods gave him "and used his solicitor for this purpose instead"; he made him "executor of his will," but unfortunately for his widow and children this solicitor "retired to the Bahamas," apparently largely on the proceeds of the winner's estate.

Although pools companies have invariably attempted to protect the interest of their winners, there have been

occasions in the past when winners have been defrauded of money partly as a result of the behaviour of individual pools representatives. Two lost money this way through being introduced at a pools company reception by one of the company's representatives to a property speculator. One of the winners concerned explained how he got involved in the fraud. The property speculator was introduced to him by the pools company's representative as an estate agent, who had helped "all big winners." When asked whether this "estate agent" was working for the pools company or not, the representative avoided a direct answer and merely stated: "He's always with us." As a result of this meeting, the winner was persuaded to buy thirty properties as an investment, with the help of the "estate agent." It later transpired, however that these thirty houses were slum properties occupied by sitting tenants whose rents were controlled, with the cost of the day-to-day repairs and maintenance exceeding the income from the rents. And it was only by an accident that the winner discovered that the properties had been previously owned by the "estate agent" himself, who in reality was a property speculator engaged in a form of fraud. The winner's solicitor had failed to inform him of the facts of the situation and the winner claimed that this was because "the solicitor was as much in it" as was the speculator, the latter having introduced his own solicitor as if he was an independent. The winner managed to persuade the pools company to stop the speculator from attending receptions, although they failed to take any action against their representative on the grounds that he was soon retiring. Only one other respondent in our main sample was prepared to admit that he had been defrauded in the same way, although he talked with reluctance about his experience and was not prepared to go into details.

Some winners lose their money through giving it away to relatives who start businesses which fail disastrously and require more and more injections of money before going bankrupt. One lady set up her sons in a business which failed because of their total inexperience.

"Two of my sons went into business, but they had an

unfortunate time; they lost a lot of money, it wasn't successful. It was the lack of experience of course – it was in the wrong area, not a big enough population. Of course I helped them – had to help them many times – but eventually it failed and they had to go back to their original work."

This winner gave money to friends and acquaintances, as well as to her relatives; she became "very disappointed in people about money": "I have given and given until I can give no more." Littlewoods offered advice, but the lady in question "didn't take advantage of it."

Many winners *are* acutely aware of the dangers of losing their money – it "haunts you a bit" – "now I have money I feel I would like to make sure I damn well hang on to it" – "what worries me I suppose basically is one day losing me money". Vivian Nicholson vividly summarized the root of this anxiety in her account of a conversation with her first husband on this subject: "I used to say, 'Oh hell, I'm going back to the sweet factory and you're going back to the pit.' He'd say, 'No, never, never, never will I be back at the pit.'"

A more frequent problem mentioned by our respondents was the feeling of boredom that many experienced after they had given up their work. Graham Eastcott describes this feeling: "I felt that I was just existing, there was no purpose in anything I did", and also how he felt this had contributed indirectly to the break-up of his marriage. A number of winners expressed the same sentiments about boredom – "I felt a useless existence" – "he started to get a little bit niggly with nothing to do" – "he just didn't know what to do with his time." One man found that this situation caused him to put on a lot of weight, having previously led a very active life with an accompanying appetite: "He piled on a lot of weight once he packed in working because he was still eating so much but he wasn't as active." To convey the feeling of apathy that sometimes goes with this lack of activity, we quote an extract from a report on a conversation with one winner; the previous day at 5.30 p.m. it had been impossible to interview him as he had been asleep:

"I therefore returned the following day at about 2.30 and

the door was opened by a very stout man with spectacles, the right-hand lense of which was badly chipped. He was wearing a yellow nylon casual shirt, open at the neck with what looked like soup stains on the front . . . he said that he didn't take much interest in things, he didn't even bother to get his spectacles repaired, he'd only just gone in that day to get them repaired as they'd been like that for months – he didn't really care about things . . . He said it took him some time to get used to not working but he spent his time now in going for walks and doing anything which occupied his mind, like crossword puzzles. He then said that he spent a good deal less time walking now, but he used to like that, and seemed to regret that he no longer did so much walking."

The interviewer went on to note that as he entered the room he found that the informant "had been watching colour television. It was a woman's programme about cooking and fashion, and presumably he had been watching that." It should be pointed out that this man was sixty-two when approached for an interview and his life-style might be said to be that of someone who had been prematurely retired, the difference being that this man had won over £100,000.

In a number of instances it appears that the boredom arising out of "having nothing to do" created quite fundamental problems in the lives of some winners; two types of problem recur: drink and divorce. Turning to drink as a reaction to boredom and a feeling of purposelessness occurs in four of the stories in the first part of the book, in the stories of Richard Taylor, Vivian Nicholson, Ethel Baker and Graham Eastcott. Yet only one of these – Richard Taylor – admitted to having been treated for alcoholism; in fact he was also the only person admitting to this among all the eighty-nine winners who formed the main part of our sample, and this compared to two people in the group of eighty-nine in the matched comparison group. There was at least one other winner who we know suffered from alcoholism, since the major cause of his death is listed on his death certificate as "chronic alcoholism." We have an account of how this came about from a friend, although being hearsay evidence, it

obviously should be treated with some caution:

"He invested the money and lived on the interest; he had nothing to do. For something to do, he took a job as a coach courier, and also went to Australia for the Test Matches, just for something to do. His wife also had nothing to do and things gradually got worse; she went to coffee mornings and then cocktail parties, and they both got to a state where they were permanently tanked up. They parted company, he went to live in a local pub, except when he used to go away for odd periods to 'dry out.' They were a perfectly happy couple who went right down the shute through having too much money."

The winner was only fifty-three when he died and it is obvious that his alcoholism significantly shortened his life. His wife divorced him and re-married soon afterwards.

The second problem of divorce looms much more largely in the statistics that we have compiled from our researches. We asked the eighty-nine respondents whether they had ever been separated or divorced and the following table is the result.

Have You Ever Been Separated Or Divorced?

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Separated	7	8%	4	4%
Divorced	13	14%	3	3%
Neither	72	81%	81	91%

Significantly, more pools winners have been separated or divorced than people in the comparison group. The reasons for this aren't as straightforward as might be imagined; it is clear from the evidence that in some cases the effects of the win contribute to the break-up of a marriage. One winner, when discussing the reasons for his divorce, stated: "Marital problems arose indirectly out of the win because the opportunity was there – although also I wasn't getting enough out of life – I went looking and found her. If opportunity had not been there, I might not have gone looking or been able to look in the right sort of areas." Other winners indicated that

the win had had similar effects: "My husband went mad, he was out all the time – never back for lunch or dinner and he started going out with other women." In some cases it is difficult to know whether the win was responsible for a marriage break-up or not; one man claimed, according to a newspaper story about his life: "I was happy before that win. I enjoyed working for things and the family were united in our efforts to get on. Then came the money." The story goes on to state that he blamed "all his bad luck on his big fortune," including the divorce from his wife; but when the latter was interviewed by us she denied this and said that the marriage was in difficulties before the win. In a case like this it is therefore possible that the winner was "rationalizing" the failure of his marriage by blaming it on the effects of the win.

An alternative pattern of explanation of the greater incidence of divorce amongst pools winners is that they can simply afford to legalise separations which, previous to their win, would have been beyond their financial means. One explicitly stated that this was the case: "I had lived with my wife for thirty years before being able to marry her; the money has meant that I could get a divorce from my previous wife and marry again." However the statistics quoted clearly indicate that there are more separations as well as divorces among pool winners, so it appears that winning the pools does have a tendency to disrupt marriages, although it must be noted that the majority (eighty–one per cent) were unaffected. Perhaps a further example of this category is the man who bought a yacht with his money and took to living in the South of France during the summer months; one informant who knew the winner well, stated that the winner separated from his wife and took his "mistress" on a tour of the West Indies on his boat and installed her in his new business back at his home town. The informant claimed that the woman in question was a "gold-digger" and had secretly re-married her ex-husband while ostensibly still having a relationship with the winner; when the latter learned of this, he "went berserk, smashed windows at the woman's house and finished up spending the night in the cell of the local

police station." The informant further stated that after this incident the winner "took to his bed" and his "mistress" came back to live with him, but he eventually died "a disillusioned man." We should treat this story with a certain amount of caution, as the informant seems to want to believe that the winner's money didn't make him any happier (he concluded his account by saying "Poor Dennis, with his £140,000 in his grave"). It should be noted in this context that the small number of winners who do get badly affected in their personal lives – feelings of boredom and futility leading to drink and a somewhat reckless pursuit of pleasure – are mostly very young at the time of their win; being young they are more likely to pursue pleasure than perhaps are older winners. This is not to say that a pursuit of the "pleasure principle," even to an extreme measure, necessarily leads to unhappiness. One young winner was reported in the press as having gone "on a world tour in search of the perfect girl," and was said later to have adopted a "playboy" way of life: red E-Type Jaguar, frequent parties, and a very rapid turn-over in girl friends. He claimed both in interviews with the media, and in a more sober interview with us, that he was perfectly happy with this style of life. This was certainly borne out by his relaxed and cheerful manner, although the rumour that has reached us since we interviewed him, that he has found his "perfect girl" in the East End of London, suggests that he has come to accept Richard Taylor's dictum that "like everything else, strawberries are nice but you can't eat four pounds of them." Also, it needs to be pointed out that the more cautious behaviour of old age doesn't necessarily lead to great happiness; as one older winner put it: "I'm too old to be very happy; you could say that I'm quite content. If younger, things might have been different."

A feeling of disillusionment with life can also arise because of a sense of isolation coming out of the rapid transformation of social circumstances. One winner who had been a miner before his win and had lived in a tight-knit mining community is quoted in one press story as saying that the win had "broken up old friendships": "Having money puts

you apart from other working people, as though you were different." The story further claimed that "he craved for the comradeship of the other colliers in his old home town." There's nobody he has met since, he says, who is a friend worth calling as such. A number of winners mentioned that old acquaintances and friends had reacted with embarrassment to the win, and this comes out in a number of stories in the first section of the book.

One of the surprising things about our overall findings is that pools winners as a whole do not feel more lonely or awkward and out of place than the sample of non-pools winners. We asked all our respondents the question, do you ever feel lonely? -- and the following was the result:

Do You Ever Feel Lonely?

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Never	61	68%	52	58%
Rarely	11	12%	14	15%
Sometimes	10	11%	23	26%
Often	7	8%	0	0%

The interesting feature of this table is that pools winners are to be found at the extreme ends of the response continuum: more of them claim to never experience loneliness and to often feel it; people in the comparison group tend to cluster more in the middle-range responses of rarely and sometimes feeling lonely. The probable explanation for this finding is that a small number of winners suffer acutely from the effects of the win in the way we have described, but the vast majority adapt themselves successfully to their new situation. A further indication that this is so comes from the answers to a question about "feeling out of place." We asked people whether they agreed or disagreed with the statement, "I often feel awkward and out of place": only thirteen per cent of the pools group said that they agreed with this, as against nineteen per cent of the comparison group.

Why most winners are able to adapt so successfully is perhaps the freedom that having such large sums of money brings. As George Ingram put it: "It changed my circumstances by removing mental and physical shackles." Some of our younger winners, becoming bored after having given up work started a whole range of businesses, or became proficient and engrossed in a variety of sporting and leisure activities. Graham Eastcott summarized the change that this made in his life, saying that "mentally I am very much more satisfied. It seems that I am around for something rather than nothing now." And of course for some winners thefreedom to give up work altogether is itself a source of great happiness, as well as lifting the strain of having to "make ends meet" and "trying to get on in the world":

"Well the money do come important to you, don't it? 'Cos if you've been working all your life, you think to yourself, well now I can take it steady, I don't have to rush here and rush there."

An indication of the exhilaration that some winners feel on giving up work comes from one winner who, after having given up his job, is quoted in a newspaper report as saying: "I've packed my job in . . . It's a great feeling knowing that I don't have to work again." This sentiment becomes even more understandable when it is realised that this winner, along with his wife, had been working an eighty-hour week as manager of his father's garage during the ten-year period previous to the win; according to the press story that reported this account of the winner's working life, he had "a seven-day working week . . . 7.30 a.m. to 10 p.m. on weekdays, 11 a.m. to 11 p.m. on Sundays." For some people the choice doesn't have to be an either/or one: one winner carried on her work as a secretary, but after a year or so did it only on mornings: "Her afternoons are given to helping her mother with housework and shopping, or taking her young niece out in the car, visiting relatives." Some winners are happy to give up work permanently without suffering from any apparent problems; one winner, when asked about how he had spent all his money, replied: "I have enjoyed twenty-five years of holiday."

Some people use winning the pools as an opportunity for both educating themselves and enjoying sporting and other activities which previously they didn't have time for; one man thought that it was self-evident that he would be happier without the "strain of working" and claimed to be unreservedly enjoying his new way of life: spending five evenings a week at night school learning French, Car Maintenance, Badminton, Painting and German, and during the day playing tennis, badminton and table tennis, as well as embarking on the self-educational enterprise of reading in alphabetical order the *Book of Knowledge* Even where a winner decides to work, he has the freedom, because of his financial situation, temporarily to abandon it in the pursuit of pleasure: "My husband used to make spontaneous visits to Jersey, flying out there for a party with our daughters; we used to have to get back by six o'clock the next morning to get ready to open up our pub." For others it gives the freedom to indulge a hobby to an intensity impossible before: one man took up bowling and became British National Champion within a year or so. This involved him travelling to countries as far afield as Sweden and the U.S.A., as well as spending between £12 and £20 a week; obviously a sport at this level is only for someone with plenty of free time and money. And there are other "freedoms" that money provides: one winner with rather traditionalist views on education decided to send his son to a private boarding school, on the grounds that "state schools allow children to play around too much." Altogether this is a "freedom" of which twenty-one of the eighty-nine members of the main pools sample availed themselves. The same consideration of money bringing choice of action applies to private medicine; a number of respondents mentioned this: "We had her in a nursing home for nearly three months . . . it made a wonderful difference to us" – "For anything serious we go private, which we did for seeing specialists." Altogether, thirteen people in the pools group stated that they consulted a doctor privately. In at least one case known to us, this had a marked effect on the health of a member of a winner's family:

"Before the win our youngest son had been physically

handicapped: he had sinus very bad, had to wear a hearing aid and his heart was on the wrong side and very slow. We had been told nothing else could be done for him, but after the win we were told by a consultant that they could help him privately. As a result of the operation the sinus was cleared and he no longer has to wear a hearing aid."

The couple were very shocked to discover that money could buy health, and felt strongly that it was very wrong that this should be possible; also they were naturally upset to discover that it had been possible all along to do something for their son and said that if they had known this before the win, they "would have struggled and found the money."

Nearly all the evidence that we have collected on health suggests that pools winners are healthier as a group than the ordinary people chosen for the matched comparison group. There is an initial period when the "shock" of the win disturbs the sleeping and eating patterns of winners, the most extreme example of this being David Llewellyn's reaction: losing two stone in weight in ten days; but after this period the health of our respondents more than returned to normal. Seventeen per cent of the pools group said that they'd seen a doctor in the previous month because of not feeling well, compared to twenty-eight per cent in the comparison group. Nearly all this difference seems to be due to physical illness, although the boundary between physical and psychologically-derived illnesses is a very thin one (it is also possible, of course, that some people in the comparison group used a visit to the doctor as an excuse to get off work). On a number of other questions about psychosomatic and psychological health, the pools winners also emerged as the healthier group; in the area of psychosomatic complaints, one difference stood out among all the others: eighteen per cent of the pools group said that they had suffered from a headache during the previous month, as against forty-two per cent in the comparison group. This finding was strongly reinforced by the answers to two other questions: twenty-eight per cent of the pools group said that they had taken aspirin-type tablets during the previous month, compared to forty-two per cent in the other group; similarly, only eleven

per cent of pools winners said that they had ever suffered from migraine, compared to nineteen per cent of the group from the general population. Our interview material gives some indication of the reasons for these differences: one woman explicitly stated "that the elimination of the strain associated with working as a tailoress had led to a significant reduction in attacks of migraine." Similarly for differences in physical health; another woman said that her health had improved after the win: she had worked as a bus station canteen worker and had to stand so much that, being overweight, she had had to have her legs bandaged when she came home from work.

The general rule for most pools winners, then, is that their health on balance appears to improve as a result of the win. In addition to the elimination of strain through giving up work the win reduces anxiety and worry through the financial security it brings. Several winners mentioned this aspect: "Winning the pools gives you security" – It frees you from financial worries" – "It takes a little bit of the worry out." It is not difficult to imagine the great relief that a young thirty-four-year-old widow living on £5.10s – £6 a week from her work as a mail order clerk (and having to keep two adolescent children) felt when she learnt that she had won £206,000 in the late 1950's: she felt "very happy" to be able to escape from her financial problems, according to newspaper reports of her win. The elimination of financial anxieties is perhaps reflected in the figures of nervous breakdowns in the pools and comparison groups: seventeen per cent of pools winners said that they had had nervous breakdowns, as against twenty-four per cent of the non-pools group. Although this difference is not statistically significant, it is certainly in the same direction as all the other answers to questions on psychological and psychosomatic health. The only evidence we have from the health point of view which contradicts this conclusion, is a finding that there are significantly more widows and widowers in the main pools group than in the comparison group. This raises technical demographic problems as to how this finding should be interpreted (and this will be dealt with in our next, more

analytical book); for the moment we can only note this somewhat puzzling finding.

Probably the reason why winners dont suffer from the dramatic transformation in their lives is that most of them soon come to terms with their new situation and adjust their way of life accordingly. An indication of this adjustment is that they maintain their moral values in spite of the win: we asked all our respondents whether they agreed or disagreed to a series of statements about people having difficulty in knowing which standards to follow; there were virtually no differences of any significance between the pools and comparison groups. For example, we asked people whether they agreed or not with the statement: "Everything changes so quickly these days that I often have trouble deciding which are the right rules to follow": fifty-seven per cent of pools winners agreed with this, but then so did fifty-seven per cent of the comparison group! When these questions were directed more personally at the respondent, the proportion agreeing with the statements dropped dramatically; for instance, when we asked whether people agreed or disagreed with the statement: "It seems to me that other people find it easier to decide what is right than I do," only twenty-one per cent of the pools winners agreed with it, as against twenty-seven per cent of the comparison group, a difference which suggests, if anything, that the pools group has less difficulty in deciding what is right and wrong than the sample from the general population. It is possible of course that the "stigma effect" of winners reacting against the myth of "unhappy winner" that we discussed earlier, is distorting our findings here, but our subjective impressions gained through meeting winners suggests that this is not the case.

Do You Ever Feel Bored?

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Never	51	57%	24	27%
Rarely	12	13%	29	32%
Sometimes	20	22%	32	36%
Often	6	6%	4	4%

Not only do pools winners come to terms with their new situation, they come to feel that their lives are less boring than they otherwise might have been. This is indicated in the table giving the results of a question on boredom.
The general pattern of answers to this question is confirmed by the responses to a similar question, which asked, "Do you sometimes feel your life is empty?": seventy-eight per cent of pools winners said "never", as against sixty-three per cent of the comparison group who gave the same reply. This is in spite of the feelings of boredom and purposelessness that a number of winners experience and that we have discussed earlier in the chapter. The resolution of this apparent contradiction lies in the fact already noted that the money gives great freedom of choice in what a person does with his life. One man, having experienced feelings of boredom after giving up work, bought a small general grocery shop and worked six days a week, twelve hours a day. Finding that this is becoming a bit tedious he is free to choose other forms of activity more to his liking:

"I do intend selling the shop and then I'll look around for something else to do; I now think I've got enough confidence to start another business. The shop was a business already established; I don't get so much satisfaction out of it as I would, for instance, if I started something on me own. I had a seven year apprenticeship in engineering, you know, and I could certainly start something in that line."

Winning the pools means that people with different temperaments can follow their own distinctive inclinations, whatever they may be. Of course, it isn't as simple as that; one found, for example, that he was much more ambivalent about the whole thing:

"I was happy before the win – happier I think sometimes – but I had a very boring job which I was glad to see the back of. Now I do the things I wanted to do, whereas I couldn't before. My chief hobby is learning the piano; it takes a lot of time but I'm interested in it. You've got to have something to do even if it's keeping fleas!"

Finally then, there is the question of whether pools winners are happier as a result of the win, and of course we

asked the winners themselves whether they thought they were: of eighty-eight people who answered this question, sixty-eight said that they *were* happier, twelve said that they weren't, and eight didn't know. Some people thought it was so self-evident that they would be happy, as to be incredulous that we had asked it; for example, one man's initial reply was, "Silly question!" This overall finding is more than confirmed by others of our findings; an even higher number said that "all things considered" they were "glad" they "won the pools": eighty said yes, this was the case, while only two said they weren't glad; four didn't know. Perhaps the most telling evidence we have on this question of happiness is the replies that pools winners gave to a question on their current happiness, compared to the answers of the comparison group:

Would You Say Your Life At The Momment Is . . .?

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Very Unhappy	1	1%	2	2%
Unhappy	1	1%	6	6%
Happy	46	51%	62	69%
Very Happy	35	39%	17	19%
Don't Know	6	6%	2	2%

The most significant difference in this table is the greater proportion of winners who said that they were very happy: thirty-nine per cent, as against nineteen per cent of the comparison group who gave the same answer. We had a very similar response pattern to a related question on disappointment with life:

Do You Sometimes Feel Disappointed With Life?

	Pools Winners		*Comparison Group*	
	Number	*Percentage*	*Number*	*Percentage*
Never	53	59%	33	37%
Rarely	6	6%	19	21%
Sometimes	24	27%	26	29%
Often	6	6%	11	12%

Significantly fewer winners said that they never feel disappointed with life, compared with the non-pools-winning group. As a follow-up to this question we asked all our respondents whether they had ever felt so depressed as to have attempted to commit suicide; only two pools winners said they had, compared to just one person in the sample from the general population. There was, however, one actual suicide among the total of 191 winners that we have information on, and although this attracted quite a bit of publicity from the press at the time, there is some doubt, whether it was related to the man being a pools winner or not. One informant who knew the winner very well said that he was "very ill after the win and did commit suicide by shooting himself; it is very difficult to establish whether this was the result of winning the pools or the illness." Similarly, the coroner who gave the verdict of suicide is reported as saying that the winner "may well have had an inner unhappiness." However, the more tangible evidence that we have leads us to suspect that it was the illness rather than the pools win that was mainly responsible for his death; neighbours were quoted by the press as saying that he "had become depressed following a stroke" four months previously, and that he'd had a second stroke just before his death. Further evidence leading to the same conclusion comes from a television interview that Alan Whicker conducted with this winner seven months before his suicide. He claimed in the interview that the win had made him happier: "It makes you happier, it's nice to have extra to what you've got, isn't it?" This was the tone of the whole interview and the winner appeared relaxed and happy throughout; he had been a very active man both in his working life and in sporting activities, but had been virtually forced to give all this up on account of yet another stroke some years previously.

Most winners do feel happier, then, as a result of winning the pools, although as we have seen, there is a considerable range and shading of feeling on this. For most pools winners, the happiness they found from their windfall was of a rather undramatic nature; as one winner put it: "I feel more secure

and able to be more venturesome and branch out into business." Others denied that they were any happier as a result of the win, claiming that they were perfectly happy before: "We were happy the way we were living, it's only brought you better things in life." Another couple stated "We're too simple to have problems. We believe in marriage, we're that sort of people." Obviously, there is a danger of over-stressing this kind of innocence, but it may be a key to understanding why most winners adapt very successfully to their situation. We might say that most people are very preoccupied in maintaining security in their lives, and that this is linked with keeping a sense of personal and social identity. One of the reasons why some winners were so badly affected by the win was that it undermined this sense of identity, but to be so affected is by no means on balance a negative matter: some of our respondents came to a new sense of personal maturity through fundamentally testing experiences of the sort described by Richard Taylor. Some winners came to feel that they had become more self-confident through the effects of their new status in life: "It gives one more confidence if you've got some money to back it up – sub-consciously you feel more self-assured."

This over-view of the evidence on happiness, raises the difficulty of working with the rather simple assumption that people can be arranged on a scale running from misery to extreme happiness. As the stories in the first section of the book brought out, the complex variety of winners responses suggests that our simplifying statistics miss the range of feelings and experiences associated with being a pools winner. But in a book of this kind we owe it to the reader to come to some conclusion, and perhaps the most appropriate one lies in a finding of our survey: more than three-quarters of the winners of our main sample said that they still do the pools.

Edward Jenner's Cowpox Vaccine:

The History of a Medical Myth

Peter Razzell

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CONTENTS

Preface to the Second Edition 3
Introduction 5
1 Jenner's Early Experience of Cowpox Inoculation 7
2 The Origin of Woodville's Lymph 15
3 The Source of Jenner's Vaccine 30
4 The Nature of Woodville's Lymph 41
5 Pustular Eruptions with Jenner's Stock of Vaccine 55
6 Benjamin Waterhouse's Use of Jenner's Vaccine in the United States and the Marblehead Smallpox Epidemic 65
7 Early Vaccination on the Continent of Europe 80
8 The Attenuation of Smallpox Virus 84
9 The Virology of Smallpox Attenuation 97
Conclusion 107
References 109
Index 123
Appendix 131

PREFACE TO THE SECOND EDITION

The publication of this second edition will nearly co-incide with the W.H.O. announcement of the total eradication of smallpox from all countries of the world. This magnificent achievement speaks for itself: the long struggle against smallpox, which includes the work of the practitioners of both variolation and vaccination, has at last reached its final fruition. The controversy about the relationship between variolation and vaccination still continues of course, and the present book is an attemp to clarify a part of that controversy. This question is not merely academic, but has a bearing on the practical task of eliminating smallpox. If, as I have argued, many of the strains of vaccinia have been derived from attenuated forms of smallpox virus, the question arises as to the status of the strains of vaccinia currently held in the world's laboratories. My belief is that at least some of them are attenuated forms of smallpox virus; for example, the strain of vaccinia preserved at the Lister Institute in London is reputed to have been derived from a Prussian soldier with smallpox in 1870.

If my argument is correct, we are in the ironic position of having eliminated the naturally occurring form of smallpox while at the same time preserving artificial attenuated strains. It is of course highly unlikely that such attenuated strains would constitute a serious hazard or form a dangerous potential source of future outbreaks of epidemic smallpox; however, it is a consideration that should not entirely be ignored, particularly in the storage and especially in the usage of existing stocks of vaccine. The danger of the latter is illustrated in a recent *Sunday Times* report on the vaccination of a pregnant woman: "Two months later, she went into premature labour. The baby was born covered in ulcers and died. At post-mortem, it was found to be heavily infected with vaccinia." I would argue that many of these cases of so-called "generalized vaccinia" are in fact forms of smallpox, and that the attenuated virus used is capable of reverting to its virulent form under special conditions. It is this possibility that makes the controversy over the origin of vaccinia virus discussed in this book of such importance.

In addition to practical considerations, there is the whole question of the scientific status of medical practice in the modern world. The elimination of smallpox is a great achievement, but if we do not really know the origin and exact nature of the virus (vaccinia) that we have been using for nearly two hundred years on many hundreds of millions of people, we should be very modest in our claim to understand the nature of medical reality. This book is partly an attempt to clarify some of these more fundamental issues as they relate to the medical history of smallpox and the prophylactic measures taken against it.

I have added a brief appendix at the end of the book to discuss one very detailed review of the first edition, that by Derrick Baxby in the *Journal of the History of Medicine and Allied Sciences.* There have been a number of reviews of my work — some favourable, some critical — but Baxby's review article is the most important because it attempts to grapple with some of the issues through a detailed examination of both the virological and historical evidence. And I have written this appendix in the belief that only a discussion of the detailed empirical evidence will help resolve the controversy generated by this book.

Peter Razzell.
December, 1979

INTRODUCTION

The thesis propounded in this book is a controversial one: it is argued that the vaccines used by Edward Jenner, after his initial trial experiments with cowpox inoculation, were derived not from cowpox but from smallpox, and that the bulk of the vaccine used for the first forty years or so of the nineteenth century was an attenuated strain of smallpox virus. Given that the inoculation of smallpox virus had been successfully practised in England nearly eighty years before his first publication on cowpox inoculation, this conclusion substantially undermines the heroic role in which the history of medicine has cast Jenner, with its emphasis on the epoch-making quality of his discovery of the prophylactic powers of cowpox against smallpox.

In spite of the controversial nature of my argument, I have attempted to avoid a polemical tone and tried to present all the relevant evidence, even where it appears to go against my case. Hopefully, the abundance of historical literature for the many countries where the early vaccines were sent, which has not always been available to me, will enable a subsequent objective evaluation of my central hypothesis. I have resisted the temptation to avoid entering the specialised field of virology, in the belief that I owe the reader an opinion on the medical interpretation of the historical findings. I have greatly benefited from talking with Professor Keith Dumbell, Dr. Alan Downie and Derrick Baxby on this aspect of the work, but in spite of the virological complexities and possibilities revealed by this discussion, I have committed myself to the hypothesis which I think both fits the evidence and is plausible, in the belief that this will provoke further fruitful discussion and work.

I would like to thank Dr. Edwin Clarke, Director of the Wellcome Institute for the History of Medicine, who kindly read the manuscript of the book and has given me general encouragement, as well as the library staff of the Institute who were of great help. My final thanks are to the Wellcome Trust for the research fellowship which enabled me to take one year off from my regular teaching duties, and

complete the work and write up the results of this book. Needless to say, all errors of interpretation and analysis are my sole responsibility.

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CHAPTER 1

Jenner's Early Experience of Cowpox Inoculation

On December 11th, 1799, Dr. Andre of Petworth in Sussex, wrote the following account of the cowpox vaccine which had been sent to him for his practice of vaccination:

> "The matter sent from Brighton to Petworth produced a disease in every shape resembling smallpox: the time of sickening, the symptoms, the eruptions and their maturation were the same. The number inoculated was fourteen. Three of these were children at the breast; the number of eruptions in them was from three to twelve. The ages of the remaining eleven were from three to fourteen, and the numbers of eruptions from fifty to a thousand."[1]

An elderly woman visiting the house in which the children were isolated caught smallpox, infected her husband, and died soon afterwards of the disease.[2] The vaccination of the children had been sponsored by Lord Egremont, one of the most influential early supporters of vaccination, and as a result of this incident, he became highly anxious about the reliability and safety of the new practice. Jenner, writing to meet these anxieties, explained the origin of the contamination of the vaccine, which in the first instance had been supplied by Dr. George Pearson:

> "About a twelvemonth ago Dr. Woodville, physician to the Smallpox Hospital, procured some virus from a cow at one of the London milk farms, and inoculated with it several patients at the Smallpox Hospital. Fearful that the infection was not advancing properly in some of their arms he inoculated them (some on the third, others on the fifth day afterwards) with smallpox matter. Both inoculations took effect; and thus, in my opinion, a foundation was laid for much subsequent error and confusion . . . Dr. Pearson . . . was then, and had been, busily employed not only inoculating from this source, but in dispersing threads embued in the virus to various

places in our own country, and to many parts of the Continent . . . In many places where the threads were sent a disease like mild smallpox frequently appeared; yet, curious to relate, the matter, after it had been used six or seven months, gave up the variolous character entirely and assumed the vaccine; the pustules declined more and more, and at length became extinct. I made a few experiments myself with this matter, and saw a few pustules on my first patients; but in my subsequent inoculations there were none."[3]

Jenner successfully convinced Lord Egremont and subsequent medical opinion that the problem of the contamination of vaccine was confined to that immediately deriving from the Woodville/Pearson lymph. Although he admitted to using it on an experimental basis, Jenner's letter implies that it did not form a main part of his own vaccine stock. I shall argue in this book, that **the bulk of the vaccine used by Jenner throughout his lifetime was the same as that which caused the smallpox outbreak at Petworth.** Evidence will be presented to show that Jenner's main stock of vaccine was derived from one of Woodville's patients who had about three hundred smallpox pustules and that this vaccine produced in the first year or so of its use cases of mild inoculated smallpox, and on at least one occasion, was probably responsible for starting a severe epidemic.

Jenner's first experiment in vaccination took place on the 14th May, 1796, when he inoculated James Phipps with cowpox taken from the hand of the milkmaid Sarah Nelmes, who lived in the neighbourhood of Berkeley. Phipps was subsequently variolated on the 1st July, and successfully resisted infection.[4] After this first trial vaccination, Jenner did not achieve further success until the spring of 1798, when more than thirteen people were vaccinated again with cowpox discovered in the Berkeley area.[5] (Some of these were vaccinated with lymph taken from the arms of those inoculated with primary cowpox — it should be remembered that until 1881 humanized lymph was the main source of smallpox vaccine in this country, and that it was only in the present

century that calf lymph became the standard mode of transmission and preservation of vaccine). Jenner did not publish details of all these cases, but it appears that on three of them "an extensive erysipelatous inflammation . . . with some degree of pain" occurred, which was treated by the application of mercurial ointment.[6] In order to prevent a repetition of this complication, Jenner applied "a little mild caustic" to the vesicle at the site of the injection of two of the children vaccinated.[7] These reactions in the first series of vaccinations were sufficiently severe to lead Jenner to hesitate about the advisability of vaccinating very young children, for at the end of the original manuscript of the *Inquiry* he wrote, "How far it may be admissible on the tender skins of infants further experiments must determine."[8]

According to Baron, Jenner's biographer, he left Berkeley for London on the 24th April, 1798 and stayed there until the 14th July, unsuccessfully attempting to interest the medical profession in his discovery. He took lymph from one of his previously vaccinated patients in dried form, and after being unable to find patients in London to try it on, he left a supply with Cline at the end of July. Cline successfully vaccinated one person with this lymph, but failed to get it to take in three subsequent cases vaccinated.[9] With this failure Jenner appears to have lost the supply of vaccine (it is not clear whether he himself took some back with him to Berkeley, and failed to propagate it). After the initial set of successful cowpox inoculations, Jenner had a series of failures in the remaining months of 1798. In the following year he wrote:

> "Four or five servants were inoculated at a farm contiguous to this place, last summer, with matter taken from an infected cow. A little inflammation appeared on all their arms, but died away without producing a pustule; yet all these servants caught the disease within a month afterwards from milking the infected cows, and some of them had it severely . . . [later] The Cow Pox appeared at a farm in the village of Stonehouse, in this county, about Michaelmas last, and . . . out of six patients that I lately inoculated two of them only were infected."[10]

Both of the two children infected with the Stonehouse lymph had ulcerated and very sore arms, and in one of them Jenner was again forced to treat the vesicle with a special ointment preparation.[11] The results were sufficiently severe for him to recommend the use of caustic in such cases:

> "Although the application I have mentioned in the case of Mary Hearn proved sufficient to check the progress of ulceration and prevent any secondary symptoms, yet, after the pustule has exerted its influence, I should prefer the destroying it quickly and effectually to any other mode. The term caustic to a tender ear (and I conceive none will feel more interested in this Inquiry than the anxious guardians of a nursery) may sound harsh and unpleasing, but every solicitude that may arise on this account will no longer exist, when it is understood that the pustule in a state fit to be acted upon is then quite superficial, and that it does not occupy the space of a silver penny."[12]

Although Jenner tried to make light of the recommendation of caustic and the anxieties that parents might feel on this score, his colleagues Woodville and Pearson believed that he had underestimated the likely strength of parental reaction on what they felt was such a drastic procedure. Pearson wrote to Jenner on the 15th February, 1799:

> "On telling Dr. Woodville that I had been anxious about your publishing the use of caustic, he replied, 'that would damn the whole business.' Be assured that if the practice cannot be introduced without the caustic, or call it by any other name, it will never succeed with the public."[13]

Woodville and Pearson did not realise at this time that Jenner had experienced very severe reactions in his primary cowpox inoculations (which he did not publish) and this only came out subsequently through information published by other practitioners. Jenner had first used the Stonehouse lymph on the 2nd December, 1798, and eleven days later on the 13th, he had allowed a neighbouring surgeon, Mr. Darke, to take some for the vaccination of five patients living in

Stroud.[14] On three of these, there was only a slight inflammation of the arm and minor scabbing — and none of them resisted variolation on the eighth day after their vaccination.[15] On the two others, there was a sore and painful inflammation of the arm, the severity of the symptoms being disputed by the two independent accounts given of the events concerned, one claiming that they "suffered severely from violent inflammation and alarming ulcerations in their arms,"[16] the other that only on one of them was the inflammation "**troublesome** and **disagreeable,** but not . . . an **alarming** and **dreadful** circumstance".[17] Yet five years later, Mr. Henry Hicks, a friend and ardent supporter of Jenner's (he had been one of the first people to have his own children vaccinated) wrote of this incident:

> ". . . I have myself been a witness to many instances of very bad sore arms in the early part of the Vaccine practice; and the Rev. Mr. Colborne of Stroud, in this county, who had two of his children inoculated, was so much alarmed at the state of one of their arms, that further medical assistance was called in, and he declared to me, that he would never have another child inoculated with Cow-pock matter. This happened at a very early period, and long before either Drs. Woodville or Pearson had interested themselves concerning it."[18]

The severity of the reaction on the arm was not the only problem that Jenner faced at this time. He had failed successfully to infect the "four or five servants" through primary cowpox inoculation during the summer of 1798, and had only effectively propagated the disease in two of the six people vaccinated with the Stonehouse lymph in December of the same year — and these had ulcerated and inflamed sore arms. Darke, who had used a supply of Jenner's Stonehouse lymph, had an almost identical experience, only being able to successfully infect two out of five, both of whom suffered from sore and painfully inflamed arms. And to complete Jenner's problems, an attempt by another surgeon also living in Stroud — Mr. Thornton — to inoculate a Mr. Stanton and his four children with the Stonehouse lymph taken indepen-

dently on the 1st December, appears to have failed in every case.[19] It is not therefore surprising that although Jenner had used the Stonehouse lymph as late as about the middle of December, he abandoned it (without ever stating why), and was unable to supply other medical practitioners with vaccine until after he had himself been supplied by Woodville in February, 1799.

One of the problems in assessing the early evidence on vaccination is the tendency that Jenner had of omitting details of failures that he was experiencing. For example, in a letter to Woodville at the end of January, 1799, he wrote that the difficulties he had with the Stonehouse matter had "not happened so generally"[20] — yet we know from evidence already considered, that there were more failures than successes up to this date. Two months later he himself admitted, "I have often been foiled in my endeavours to communicate the Cow Pox by inoculation."[21] It is likely that there were cases of failure not mentioned except indirectly; although he implied in the *Inquiry* that there were no outbreaks of cowpox in 1797[22] (Baron, later explicitly stated that cowpox had disappeared from the dairies between the spring of 1796 and the spring of 1798[23]), in a publication at the end of 1799, Jenner referred to "matter with which my inoculations were conducted in the years 1797, 1798, 1799, was taken from different cows."[24]

It is now known that the inoculation of primary cowpox is very difficult to achieve, and that success is the exception rather than the rule. Estlin, who was concerned about the deteriorating quality of vaccine through arm-to-arm passage, made extensive enquiries, and engaged in a great deal of correspondence with people working on the problem of primary cowpox inoculation and came to the conclusion that "matter taken from the cow, and inserted into the human subject in the ordinary method with a lancet, seldom reproduces the disease."[25] A year or so later, at the end of the 1830s, this was confirmed by Ceely who undertook a series of detailed experiments, the conclusions of which were summarised as follows:

"1. More than half my attempts to vaccinate with primary

lymph [cowpox directly from a cow], taken from vesicles at a proper stage, and possessing all the characteristics of perfection, have entirely failed. The same individuals have immediately afterwards been successfully vaccinated with dry or liquid lymph which had been long current in man.

2. A small number, vaccinated from the same primary sources, afforded results in various degrees of imperfection . . . Nearly all these subjects have been successfully re-vaccinated with ordinary lymph, from periods of nine to eleven months . . .
3. A still smaller number, vaccinated from the same primary stocks, have furnished vesicles in the highest degree of beauty and perfection. But even in many of these there has been more or less delay in the full development of the vesicles; and in nearly all, the number of vesicles has seldom equalled one-half of the punctures.
4. Precisely similar phenomena of entire failure, imperfect or complete vaccination, with all their attendant circumstances, have followed the use of lymph from perfect casual vesicles on the hands of the milkers; and the like results have frequently attended the early removes of lymph from the most perfect primary vaccinations."[26]

Ceely also noted the initial severe reactions to those primary vaccinations that did take, and described them as follows:

". . . it too often happens, especially in subjects with thin and vascular skins, that the vesicles burst or are easily broken during the height or about the decline of the areola; and if the subject be of a strumous or erysipelatous diathesis, of full habit, and possess an irritable skin, secondary inflammation is set up and becomes more diffused and deeper seated, the corium is destroyed completely, and a slough of the subjacent tissue is soon manifest, the surrounding integuments are deeply indur-

ated . . . All this mischief, however, generally soon subsides . . ."[27]

Other workers in this field, however, did not take such a sanguine view of these complications; for example, Bousquet, who was one of the first to inoculate primary cowpox after the Jennerian period, gave the following description of his initial work:

"In my first trials with the new virus (cowpox taken from a milkmaid), I made, according to my custom, three punctures on each arm. I soon had to give up this practice. The intensity of the inflammation was sometimes so great that it spread over the entire arm as far as the glands of the axilla. M. Gasc cannot have forgotten a child who he had vaccinated and had the kindness to show me. The vesicles were enormous, the inflammation so violent that . . . The crusts, when they fell off, left ulcerations which were very slow to undergo cicatrisation. It was at this moment that I understood, for the first time, Jenner's anxieties."[28]

We see, therefore, that Jenner's experience with inoculating cowpox up to the end of 1798 was typical of what was to be found by later workers: frequent failures of the injection to take, and in those inoculations which were successful, occasional severe ulceration and inflammation of the arm.

CHAPTER 2

The Origin of Woodville's Lymph

By the end of 1798, Jenner had performed only about a dozen successful vaccinations, most of which had taken place in the spring of that year. Although several London physicians, including Woodville and Pearson, had approached him for vaccine, Jenner was unable to supply any because of the failure of the Stonehouse lymph and the unavailability of any other sources. Towards the end of January, 1799, an outbreak of cowpox was discovered at a London milk farm in Gray's Inn Lane, and Woodville, physician to the London Smallpox Hospital, was informed of this; on the 21st January, accompanied by Thomas Tanner, a veterinary student from Gloucestershire and a friend of Jenner's, he visited the farm. After Tanner confirmed that the disease was a genuine case of cowpox, Woodville inoculated six people with the virus, and two or three days later, inoculated eight other people with virus taken from the pustules of milkers, after comparing them with plates in Jenner's *Inquiry*.[29] This dual confirmation of the similarity between the Gray's Inn Lane strain of cowpox and that previously experienced by Jenner is important, and it makes it very unlikely that Woodville inadvertently introduced a very severe form of cowpox (there is some evidence, although of unknown reliability, that such severe strains did possibly exist[30]). It is conceivable that one of the milkers accidently lodged smallpox virus on the udders of the cows — it appears that smallpox virus can be passaged in this manner without loss of virulence[31] — although the similarity of the symptoms on the Gray's Inn Lane cows with those observed by Tanner and described by Jenner from their experience with Gloucestershire cowpox, again makes this unlikely. The most important evidence in support of this conclusion, however, is that Woodville experienced problems of contamination with all of the different number of early strains of cowpox that he introduced into the London Smallpox Hospital, where the bulk of his vaccinations took place.

This hospital treated cases of natural smallpox, as

well as practising the inoculation of smallpox (variolation). This fact alone, seriously complicates the problem of interpreting the results of these early trial vaccinations: some of the people attending the hospital for vaccination resorted to that measure only because of their previous contact with a case of natural smallpox, while others would be at risk from catching the natural disease through contact with patients and staff at the hospital. Woodville was aware of the latter risk, and in his report on the first 500 vaccinations, he wrote:

> "Among the patients inoculated for the Cow Pox during the first week in which I obtained the matter of this disease, several were so circumstanced as to be afterwards constantly exposed to the infection of Small Pox. Having no proof that the progress of the infection of the former would supersede that of the latter, I used the precaution to inoculate the patients with variolous matter on the fifth day after that taken from the cow had been inserted."[32]

Jenner appears to have been mistaken in his letter to Lord Egremont, when he claimed that Woodville variolated these early patients on account of being "fearful that the infection was not advancing properly in some of their arms." Woodville published detailed case studies of the first 200 people he vaccinated: of the initial ten cases inoculated with primary cowpox, only one had no reaction at the site of vaccination within the first five days. The latter was variolated on the third day; of the nine others, six were variolated on the fifth day, two on the eleventh day after vaccination and one not at all. Six of these first ten cases had pustular body eruptions strongly resembling smallpox. It is now known that a full antibody response takes about ten days, and as seven of these people had been variolated within five days, it is not surprising that some of them responded to their variolation. However, this could not explain all the events which took place; the one person not variolated at all (the third case) had a reaction almost identical to that of inoculated smallpox, finishing up with twenty-four smallpox-type pustules, and although a smallpox inoculation would take full effect within nine or ten days

of a previous vaccination, its effect would be significantly modified. The Vaccine Pock Institution conducted a series of experiments on this subject and found that two days after vaccination, variolation produced only a local effect; previously Robert Willan had found that although there was a general pustular reaction within the first nine days, the pustules rarely maturated.[33] Also, even more difficult to explain, future cases vaccinated with material taken from the site of the vaccination of some of these first ten cases, had numerous pustular eruptions, in spite of not themselves being variolated within the first ten days or so. At the end of the series, nearly two-thirds of Woodville's first five hundred cases had had pustular eruptions other than at the site of the vaccination.

This experience was in total contradiction to Jenner's findings about the results of primary cowpox inoculation:

> "Inflamed spots . . . appear on different parts of the hands of the domestics employed in milking, and sometimes on the wrists, which quickly run on to suppuration . . . these superficial suppurations put on a circular form . . . The lips, nostrils, eyelids, and other parts of the body, are sometimes affected with sores; but these evidently arise from their being needlessly rubbed or scratched with the patient's infected fingers. No eruptions on the skin have followed the decline of the feverish symptoms in any instance that has come under my inspection, one only excepted, and in this case a very few appeared on the arms . . . in the Cow-Pox, no pustules appear . . ."[34]

Jenner's conclusion was based, however, on a very limited experience, and must be compared with that of Ceeley who had acquired by 1842 experience of thirteen primary cowpox vaccines (although seven of these were derived from inoculating cows with smallpox virus). Ceeley concluded from this experience "that we have no better standard of comparison of the local and constitutional symptoms of efficient vaccine than that originally furnished and so beautifully illustrated by Jenner."[35] He was of course referring to Jenner's *Inquiry*, published in the summer of 1798, before Jenner had received a stock of Woodville's lymph. Although Ceeley's own

experience of cowpox inoculation indicated that in the main it led to a purely local reaction at the site of the injection as described by Jenner, he did note that with two of the vaccines derived from inoculating cows with smallpox, there were minor eruptions of a non-variolous character:

> "In no adults, except in the case of my assistant, Mr. Taylor, was there any attendant eruption; nor in any child the slightest approach to anything of a varioloid character. Roseola, stophulus, lichen, were the principal eruptions . . . but small **eruptive** supernumery vesicles were observed in several cases at the period of full development of the areolae, and within its sphere, when **points only** were used. In one case a vesicle appeared on the shoulder, and one on the neck. In two other cases two vesicles appeared on the abdomen, all during the early removes of both stocks of lymph."[36]

In his discussion of the inoculation of natural cowpox lymph, Ceeley did not specify the occurrence of such eruptive supernumery vesicles, although he did state that "roseola" and "lichen" occurred in some cases.[37] He did manage to produce supernumery vesicles with such lymph, but only by injecting massive doses of vaccine and mixing it with the blood from the site of the injection — and then the supernumery vesicles only arose where the mixture had dried, although "sometimes they will appear even two or three inches distant from the puncture, when the blood, mixed with the lymph, has trickled down and dried there."[38] By adopting this procedure he was able to produce fifty such vesicles in a total of one hundred and fifty cases, although apparently he was unable to produce eruptive vesicles.[39] It would appear therefore that genuine cowpox inoculation very rarely produced secondary eruptive pustules, and that Jenner was correct in the conclusions he reached in the *Inquiry* about this matter.

Woodville initially assumed that taking vaccine from the site of a primary cowpox inoculation, was valid, even if there were smallpox-type pustular eruptions on other parts of the body, on the grounds that vaccination would remain a purely local disease, unaffected by the process of variola-

tion.[40] Willan's experiments appeared to confirm these assumptions: cases in which variolation followed vaccination within nine days produced both vaccine vesicles at the site of vaccination, and modified smallpox pustules at the site of variolation and elsewhere. Material taken from the vaccine vesicles produced a purely local reaction typical of classical vaccination, whereas virus taken from the smallpox pustules produced results identical to inoculated smallpox.[41] In Woodville's cases, however, lymph taken from the sites of primary cowpox inoculation produced numerous pustular eruptions on subsequent cases [42] suggesting that he had somehow contaminated these sites of cowpox inoculation with smallpox.

What was the source of this contamination? One explanation which was immediately suggested by contemporaries was that the lancets used for the primary vaccinations had been themselves contaminated. This was Woodville's own immediate suspicion, but he discounted this possibility in his first publication:

> ". . . when I first observed a pustular eruption on Buckland (case three), the occurrence being wholly unexpected, I was not without apprehension that the lancet that was employed in its inoculation might have had some particles of variolous matter adhering to it. But this suspicion was soon removed, for, upon enquiry, I found that all the lancets which I had used on the 21st January were then made use of for the first time since they had been ground at the cutler."[43]

This does not eliminate the possibility that the lancets were contaminated, as before they were reground they would have been used for variolation in the hospital, and may not have been cleaned sufficiently to remove all traces of smallpox virus. However, there are other reasons for believing Woodville was right when he discounted contaminated lancets as the explanation of the pustular eruptions. In a further report published in 1800, he stated:

> "At various times I procured the vaccine virus, as produced in different cows, and with it inoculated patients in

the Hospital; but the effects of all the matter I tried were perfectly similar: and pustules proved to be no less frequently the consequence of these trials than of those made with the matter formerly employed."[44]

It is highly unlikely that after having been made aware of the possibility of contaminated lancets at the beginning of the series of trial vaccinations that Woodville would have subsequently used contaminated lancets in all of his trial primary cowpox inoculations. Also, the timing of the first cases in the series is strong evidence against the lancet contamination hypothesis. The relevant details of the six cases inoculated with primary cowpox in the first week who had pustular eruptions are as follows: Case Three: not variolated, 24 pustules, starting on the eleventh day after vaccination; Case Four: variolated twice "during the progress of the Cow Pox infection", five pustules appearing on the fifteenth day; Case Five: variolated on the third day after attempted vaccination, four pustules on seventeenth day; Case Six: variolated on the fifth day after vaccination, 170 pustules beginning to appear on the thirteenth day; Case Eight: variolated on the fifth day, 300 pustules starting on the fourteenth day; Case Nine: variolated on the fifth day, three pustules appearing on the twelfth day.[45] The average period for pustules to appear after variolation is about nine to ten days[46] (although as Woodville and others pointed out, "this frequently happens much sooner or later"[47]), whereas the average period after the attempted inoculation of cowpox when the pustules appeared in these six cases was fourteen days. Given that most of them were variolated on the fifth day, this suggests that the pustular eruptions were the result of the variolation and not of the primary cowpox inoculation, contaminated through lancets with smallpox matter on them.

The above conclusion still does not explain why subsequent vaccinations, using material from the sites of attempted primary cowpox inoculation, also produced smallpox pustular eruptions. None of these subsequent cases were variolated before the tenth day and not only this, the other experiments that Woodville conducted attempting to inoculate

primary cowpox from other cows produced pustular eruptions, even though "no variolous matter was applied during the whole progress of the infection." It is probable that the solution to this problem is as follows: (1) all Woodville's cases were constantly subjected to exposure of smallpox virus, either directly through early variolation for the cases in the first week, or indirectly for the latter cases, through natural smallpox infection within the confines of the hospital; (2) as has been seen, it is very difficult to inoculate primary cowpox and the probability is that many of Woodville's primary vaccinations did not successfully take, but merely created abrasions and cuts on the skin, with a purely local inflammation; (3) Dixon in his recent textbook on smallpox, has pointed out that such abrasions and cuts make ideal sites for the development of smallpox infection, and has speculated on the possibility that some apparently successful vaccinations during smallpox epidemics may have been just such infection of skin abrasions and cuts with smallpox virus.[48] This argument is supported by contemporary evidence on cross-infection; for example, Ring quoted the following case involving one of Jenner's nephews: "He inoculated a person with vaccine matter, and also with variolous matter, at about two inches distance . . . In this patient to whom it [the matter from the vaccine vesicle] was immediately transferred, a local pustule was only excited; but in a subsequent one, the taint appeared, and an eruption of about 300 variolous pustules took place."[49] And similarly, Willan, in his discussion of the contamination of vaccine with smallpox virus pointed to experimental evidence along the same lines:

> "When variolous matter is inserted eight or ten days before Vaccine Inoculation, the fluid in the Vaccine Vesicle becomes purulent, or is mixed with pus, after the tenth day, and in this state, according to the observation of Mr. Wachsel [of the London Smallpox Hospital] it will sometimes communicate the Small-pox only, but not always in the mildest form."[50]

The smallpox infection in the above case took place some days before the attempted vaccination, but the principle is the

same — the secondary infection of the site of the vaccine injection with smallpox virus.

Woodville's vaccine displayed virtually none of the qualities of cowpox vaccine as described by Jenner: it produced numerous pustular eruptions, it successfully infected all those inoculated with it, and as Woodville himself noted, it produced a very different local reaction to that described by Jenner:

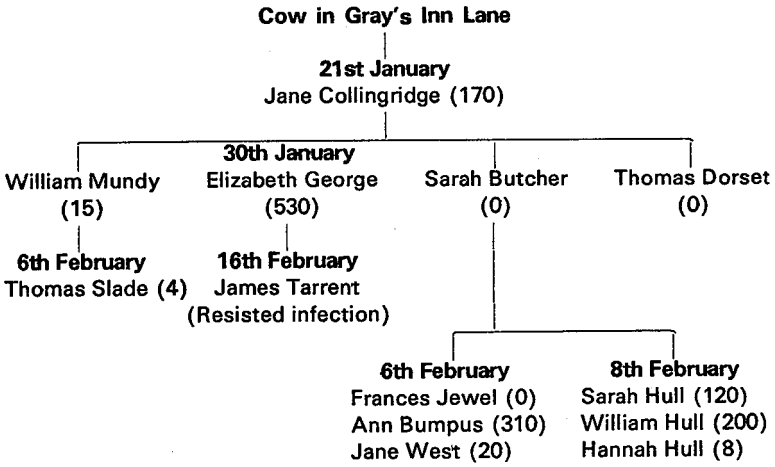
> "We have been told that the Cow Pox has frequently produced erysipelatous inflammation and phagedenic ulceration; but the inoculated part has not ulcerated in any of the cases which have been under my care, nor have I observed inflammation to occasion any inconvenience, except in one instance . . ."[51]

There was also a difference in the colour of the tumour at the site of injection: Jenner had noted from his early cowpox inoculations that this was "a colour distantly approaching to blue", whereas Woodville claimed that this was "an appearance which the inoculated disease never assumes."[52]

On the 15th February, 1799, Pearson sent Jenner some of Woodville's lymph dried on a thread, enclosed in an accompanying letter. According to Woodville, "the matter sent was taken from the arm of Ann Bumpus, who had three hundred and ten pustules, all of which suppurated."[53] As I will be arguing later that this lymph was the source of Jenner's main stock of vaccine throughout the remainder of his lifetime, it is of some importance to discuss in detail the history of this particular strain, through an analysis of Woodville's case histories. This strain was one of the two that Woodville passaged by arm-to-arm inoculation to form the basis of what was to become known as the "world's lymph". The detailed discussion of this vaccine will illustrate the symptomatic nature of the cases involved, allowing the reader to judge for him or herself the quality of this part of the evidence.

The following is a tabular representation of the genealogy of the virus with which Ann Bumpus was inoculated; I have taken the series from the initial inoculation from

the cow on the 21st January through all cases vaccinated to Bumpus and her contemporaries. The number of pustular eruptions are indicated in brackets following the names of the cases concerned, and the date of the inoculation is also indicated.[54]



Woodville's description of the primary vaccination of Collingridge was as follows:

> "Jane Collingridge, a healthy active girl, seventeen years of age. **Third Day:** The inoculated part began to be elevated and inflamed. **Fifth Day:** It was vesicated, and attended with itching. She was inoculated with variolous matter in the right arm, the former [cowpox] inoculation having been in the left. **Eighth Day:** The whole tumour is much increased in all dimensions; its form is perfectly circular, and it appears of a lemon-coloured tint. She now complains of a stiffness across her arms, and of a pain in the left axilla; the puncture in the right arm begins to be elevated and inflamed. **Eleventh Day:** She complains of a headache and pains about the loins; the tumour produced by the Cow Pox matter is now more inflamed at the margin, which is beset with minute confluent pustules, the variolous tumour is also advanced to a state of vesti-

cation; and she reports that last night both axillae were painful. **Twelfth Day:** She continues indisposed; the tumour is surrounded by an extensive efflorescence; the variolous tumour is of a deeper red colour. **Thirteenth Day:** The Cow Pox tumour is subsiding and forming a scab; that of the Small Pox is efflorescent; her headache continues; pain in the right axilla; several pustules appear. **Fifteenth Day:** There are small pustules round the edges of the variolous tumour; more pustules appear scattered over the face, body and limbs. **Seventeenth Day:** The scab over the Cow Pox tumour is completely formed; at its edges however, a fluid is still visible; the variolous tumour is in a state of suppuration; she complains of a sore throat, the number of pustules is now from one to two hundred, in no respect different from variolous pustules of the mild sort. From this time both the tumours gradually healed, and the pustules dried at the usual time."[55]

There is no doubt from this account that Collingridge had been very effectively variolated with all the classical symptoms of a case of inoculated smallpox. The only question is the nature of what Woodville called the "cow pox tumour". It appears that on the eleventh day after attempted primary vaccination there was further infection of this site of injection, with additional inflammation and more pain in the axilla. The fact that Collingridge had had 170 pustules "in no respect different from variolous pustules of the mild sort", and that these pustules had maturated ("the pustules dried at the usual time") strongly suggests that the primary cowpox inoculation had not taken in such a way as to modify the effects of the subsequent variolation. It is difficult to resist the conclusion that the "cow-pox tumour" had in effect become a smallpox one through secondary infection.

Four people were vaccinated from Jane Collingridge on the 30th January (four days after she had been variolated): William Mundy, Elizabeth George, Sarah Butcher and Thomas Dorset. The following are extracts from the published case notes which are relevant to the present argument:

"William Mundy . . . **Fourteenth Day:** Several pustules

appeared upon his neck and back, but disappeared in two or three days without suppurating.
Elizabeth George . . . **Fifteenth Day** . . . some pustules appear on her face. **Sixteenth Day** . . . more pustules show themselves . . . **Eighteenth Day:** More pustules have appeared . . . **Twentieth Day:** the pustules are very sore, and in a purulent state; their number is five hundred and thirty . . .
Sarah Butcher . . . **Fourteenth Day:** no eruption.
Thomas Dorset . . . About the **Twelfth Day** this man had four or five pustular appearances which he called pocks, but they seemed to me more like common pimples than variolous pustules."[56]

All three cases where pustules or pustule-like pimples appeared, were variolated, but not until after the pustules had appeared, i.e., from the thirteenth day onwards. The pustules erupted a little later than might be expected, assuming that the vaccine being injected was smallpox virus,[57] but as has been previously pointed out, there is considerable variation around the average time of pustular eruption. It is interesting to note that Woodville discounted the pustule-like pimples of Thomas Dorset (in the summary return of symptoms, he listed him as having no pustules), a tendency to play down ambiguous symptoms which was typical during the period of early controversy about the nature of vaccines in use.

On the 6th February, Thomas Slade was inoculated from the arm of William Mundy:

> "**Eleventh Day** . . . three or four pustules appear; the tumour is bordered with small confluent vesicles . . . **Nineteenth Day** . . . The pustules do not suppurate and are receding. **Twenty-second Day** . . . He was inoculated with the matter of Small Pox, which produced a redness for two or three days and afterwards gradually disappeared."[58]

This case displayed all the symptoms of a mild form of inoculated smallpox — the timing of fever, the appearance of eruptions, etc., and this was a direct function of the ostensible vaccination, as explicit variolation did not take place

until the twenty-second day. Ten days after the inoculation of Slade, James Tarrent was vaccinated from "a pustule upon Elizabeth George". This was the first example of what became fairly common in Woodville's early practice — taking vaccine from eruptive body pustules which were indistinguishable from those of inoculated smallpox. (Woodville at this time came to believe as a result of these trial vaccinations that inoculated cowpox was a pustular disease resembling in many respects classical variolation). There was no reaction to either the attempted vaccination or a subsequent variolation; Woodville considered "this man as one of the few whose constitutions cannot be affected by either the virus of the Cow Pox or the Small Pox."[59]

We can now return to the group of inoculations of which Ann Bumpus, the source of Jenner's vaccine, formed a part. On the 6th February, Frances Jewel, Ann Bumpus and Jane West were all injected with virus taken from the arm of Sarah Butcher, who had had no pustular eruptions herself, and had been directly vaccinated from Jane Collingridge. No pustules appeared on Frances Jewel, but on Jane West twenty-two pustules all of which suppurated, started to appear on the fifteenth day.[60] The following is a verbatim account of the inoculation of Ann Bumpus:

> "Ann Bumpus, aged twenty years, was inoculated February 6th with the matter of Cow Pox, taken from the arm of Sarah Butcher. The appearance of the inoculated part in this girl's arm corresponded in every respect with those stated in West's case. **Eighth Day:** She complained of headache. **Tenth Day:** Pain of the head and loins; shivering. **Eleventh Day:** Two or three pustules appear upon her face. **Thirteenth Day:** Pains continue; more pustules appear. **Fifteenth Day:** No complaint; the pustules were counted and found to be three hundred and ten, resembling those of Small Pox. **Seventeenth Day:** Complains of sore throat. **Nineteenth Day:** Pustules drying. **Twenty-second Day:** Inoculated with the matter of Small Pox, but no inflammation was produced by it."[61]

Given that all three hundred and ten of these pustules sup-

purated, Ann Bumpus must have been inoculated with smallpox and not cowpox vaccine on the 6th February, and therefore the virus taken from her arm to send to Jenner on the 15th February was a strain of smallpox virus. Two days after her vaccination, Sarah, William and Hannah Hull were inoculated with the same virus: all had pustular eruptions (120, 200 and 8 respectively), the timing of which were typical of classical variolation.[62]

Soon after these inoculations, Jenner received the thread from Pearson, and vaccinated several children with it. On the 13th March, 1799, he wrote the following letter to Pearson:

> ". . . Twelve patients have since been inoculated with matter produced by this virus. They all took the infection. This is the ninth day, and they appear a little ill — no eruptions yet. The character of the arm is just that of cow-pox, except that I do not see the disposition in the pustule to ulcerate as in some of the former cases . . ."[63]

Baron states that Jenner received Woodville's lymph from Pearson "in the early part of March",[64] but as the latter had sent it in the letter dated the 15th February and Jenner had vaccinated his nephew Stephen Jenner and the boy James Hill before inoculating eighteen other people with matter from Hill's arm,[65] it is likely that the first two vaccinations took place some time during the last week of February.

It should be remembered that these were, in effect, the first set of vaccinations that Jenner had successfully performed since the initial dozen or so cases in the spring of 1798; unlike the attempted primary cowpox inoculations of the summer and winter of 1798, this new series were successful in every case, and showed no signs of painful inflammation and ulceration of the arm. Although Jenner made the general claim that Woodville's lymph produced similar results to his previous vaccinations, the specific accounts of this new vaccine were very different. The only detailed case histories that he published were for the two children first inoculated with it:

"**Stephen Jenner,** three and a half years old . . . 8th [day-

Arm] increasing in elevation. A few spots now appear on each arm near the insertion of the inferior tendons of the biceps muscles. They are very small, and of a vivid red colour . . . 10th. The spots on the arms have disappeared, but there are three visible in the face. 11th. Two spots on the face are gone; the other barely perceptible. 13th. The pustule delineated in the second plate in the Treatise on the Variolae Vaccine, is a correct representation of that on the child's arm, as it appears at this time. 14th. Two fresh spots appear on the face . . . **James Hill**, four years old, was inoculated on the same day, and with part of the same matter which infected Stephen Jenner . . . 10th [day]. The vesicle more elevated than I have been accustomed to see it, and assuming more perfectly the variolous character than is common with the Cow Pox at this stage. 11th. Surrounded by an inflammatory redness, about the size of a shilling, studded over with minute vesicles . . ."[66]

Although Jenner referred here to the eruptions on his nephew as spots, in some later publications he described them as pustules which failed to maturate. The discussion of Jenner's experience of pustules resulting from the use of his vaccine will form a major part of this book and will be dealt with in a later section; for the moment it must be noted that the pustule-like spots on the first case and the nature of the tumour of the second, indicated the variolous origin of Jenner's supply of Woodville's lymph.

In addition to admitting that the new vaccine led to less elevation of the tumour, with further experience of Woodville's lymph, Jenner summarised the following additional differences:

"The principal variation perceptible to me in the action of the vaccine virus generated in London, from that produced in the country, was its proving more certainly infectious, and giving a less disposition in the arm to inflame. There also appears a greater elevation of the pustule above the surrounding skin. In my former Cases, the pustule produced by the insertion of the virus was more like one of

> those which are so thickly spread over the body in a bad kind of confluent Small Pox. This was more like a pustule of the distinct Small Pox, except that I saw no instance of pus being formed in it, the matter remaining limpid till the period of scabbing."[67]

Thus, Jenner's experience with Woodville's vaccine was very different from his own with cowpox vaccine. One of the most important aspects of this difference was the success of the former in infecting people through inoculation; Woodville stated that Jenner had written to him acknowledging that his lymph "had succeeded better than any of the vaccine matter which he [Jenner] had tried before."[68]

CHAPTER 3

The Source of Jenner's Vaccine

The success of the Woodville lymph is of great importance in understanding the history of Jenner's own use of vaccines. The certainty of infection, the absence of ulceration and painful inflammation of the arm, and the fact that it was the first stock of vaccine to be successfully propagated beyond a limited series of initial cases, were probably all factors in explaining Jenner's future practice in its use. This does not mean that he stopped looking for independent sources of vaccine at this time — in his letter to Pearson on the 13th March, 1799, he still expressed frustration at the absence of cow-pox in the countryside, with the exclamation that there was "No cow-pox yet in the country!"[69] With the vaccine that he had from Woodville, he inoculated twenty children in the Berkeley area[70] and in the same month of March allowed Dr. Marshall of Eastington to take matter from the arm of one of his patients. Between the 22nd March and the 26th April, Marshall inoculated 107 people with it[71] and it is probable that Jenner sent out this lymph to other practitioners at about the same time.[72] According to Baron on the 21st March, Jenner left Berkeley for London, to take up residence there so as to establish his claims and reap the benefits of being the discoverer of vaccination; he appears to have stayed there until the 14th June.[73]

Baron claimed that Jenner became preoccupied at this time with the problem of the contamination of Woodville's vaccine and as a result acquired a fresh source during the month of April:

> "As the events which occurred at the Small-Pox Hospital were so different from what he had experienced, he was very desirous of procuring fresh cow-pox virus from the country. For this purpose he sent to Gloucestershire; and by great exertions on the part of Mr. Robert Tanner, he procured some from North Nibley. A portion of this he gave to Mr. Knight on the 12th April."[74]

Baron does not explicitly state the year in which this occurred

but placed the comment in a sequence which suggests he was referring to the year 1799. He was probably mistaken in this respect, and his specification of dates of events can be shown to be in error for this period. He stated that Jenner had left Gloucestershire on the 21st March, yet the latter had dedicated his second pamphlet from Berkeley on the 5th April. It is quite possible that Jenner was not even in London on the 12th April when he is supposed to have given Mr. Knight this vaccine. More importantly, neither Jenner, Ring, Pearson, nor Woodville, who all discussed the relationship between various vaccines during this early period, mentioned the North Nibley lymph. Jenner undoubtedly would have discussed it if it had been successful, because of his concern for the difference between London and country viruses; in his second pamphlet he claimed that "further experiments made in London with vaccine virus generated in the country, must finally throw light on what now certainly appears obscure and mysterious",[75] and as this was written at the beginning of April, 1799, he would have certainly referred to the North Nibley vaccine in his third pamphlet at the end of the year, if he had used it successfully in 1799. (He discussed in the third pamphlet the differences between London and country vaccines at some length.).

Baron, later in his biography, makes further reference to a vaccine which is probably the same as that above:

> "On the 12th April [1800] Dr. Jenner received, whilst in London, some matter which had been generated on the cow by inoculation with the virus of grease by Mr. T. Tanner. Some part of this matter he transmitted to Mr. Wachsel, of the Small-Pox Hospital."[76]

The date that Baron refers to in both the above accounts is the same — the 12th April — and in both cases the virus was procured via a member of the Tanner family. Thomas Tanner had been apprenticed to his uncle, Robert Tanner, a "cowleech" and farmer at Rockhampton in Gloucestershire, before studying in London under Mr. Coleman.[77] John Ring made several references to the vaccine sent by Thomas

Tanner to Jenner in 1800 but none to one in 1799.[78] It is, however, possible that Robert Tanner had sent cowpox virus to Jenner in the spring of 1799 which failed to take on inoculation, and that like earlier failures, Jenner simply failed to mention it in his published writings.

Baron may have been confused by the fact that Jenner did acquire a fresh source of vaccine in the spring of 1799 with the help of Thomas Tanner, but it came not from Gloucestershire, but from London:

> "One experiment, tending to elucidate the point under discussion [pustular eruptions with Woodville's London vaccine] I had myself an opportunity of instituting. On the supposition of its being possible that the cow which ranges over the fertile meadows in the vale of Gloucestershire, might generate a virus differing in some respects in its qualities from that produced by the animal artificially pampered for the production of milk for the metropolis, I procured, during my residence there in the spring, some Cow Pock virus from a cow at one of the London milk farms. (It was taken by Mr. Tanner, then a student at the Veterinary College, from a cow at Mr. Clark's farm at Kentish Town.). It was immediately conveyed into Gloucestershire to Dr. Marshall, who was then extensively engaged in the inoculation of the Cow Pox . . ."[79]

In a later letter to Ring, Jenner mentioned that this London outbreak of cowpox had occurred "some time in April",[80] but it must have been at the end of April, because Marshall in a letter to Jenner dated the 26th April, 1799, makes no mention of it and only discussed vaccinations made with it in a second letter dated 8th September.[81] Jenner was clearly concerned with the problem of explaining the pustular eruptions resulting from Woodville's lymph; that he could entertain what, in retrospect, can now be seen as a scientifically unfounded notion about the differences between London and Gloucestershire cowpox, only illustrates how confused he was at this time.

Although Jenner had by the end of April acquired an

additional source of vaccine to that which he had obtained from Woodville, it is important to realise that **he did not use it himself** in his London practice, but had sent it to Marshall in Gloucestershire for the experiment described above. He made a number of references in his letters and publications to the use of the Kentish Farm vaccine which allows us to trace its history. At the end of 1799, he wrote an account of developments between the spring and the end of the year, and in that publication quoted letters from Marshall to illustrate the effects of using the new vaccine. Unfortunately, he confused the issue by (inadvertently?) claiming that Marshall's first letter described the results of the Kentish Town lymph, when in fact at the time when it was written (26th April, 1799) Marshall was still exclusively using Woodville's vaccine.[82] In a second letter, dated the 8th September, Marshall did in a postscript mention the new vaccine and stated that he had inoculated 127 people with it.[83] Three weeks previous to this Jenner wrote to Ring (on the 16th August) and included an account of the Kentish Farm lymph:

> ". . . Some time in April the cow-pox appeared at one of the great milk farms in the neighbourhood of the town. With this virus several patients in the country were immediately inoculated . . the cases now amount to more than seventy."[84]

As we saw earlier, Jenner was living in London from April until the middle of June, and from the numbers mentioned by him and Marshall, it would appear that Jenner himself was not using this virus by the autumn of 1799. On the 27th November, Jenner wrote to De Carro with further mention of the lymph and stated that "upwards of two hundred Persons have been inoculated from this source."[85] The first explicit reference to Jenner using it himself occurred in his pamphlet written at the end of 1799:

> "I have myself inoculated a very considerable number from the matter produced by Dr. Marshall's patients, originating in the London cow . . . and have dispersed it among others . . . From this source, Mr. H. Jenner informs

me, he has inoculated above a hundred patients . . . Whether the nature of the virus will undergo any change from being farther removed from its original source, in passing successively from one person to another, time alone can determine. That which I am employing has been in use near eight months, and not the least change is perceptible in its mode of action either locally or constitutionally. There is therefore every reason to expect that its effect will remain unaltered, and that we shall not be under the necessity of seeking fresh supplies from the cow."[86]

In this passage, Jenner very clearly contrasts his limited use of the Kentish Farm lymph with that of his main stock which had "been in use near eight months". The date when Jenner wrote the pamphlet in question is unknown, but it must have been before the end of 1799 because he refers at the beginning to Woodville having started his work on vaccinations "in the early part of the present year".[87] According to Baron, he wrote a letter "in the early part of December" in which he referred to publishing an appendix attached to the new editions of his first two pamphlets.[88] This appendix was issued with a separate title but bound with his first two works, the whole having a dedication to the King and dated the 20th December, 1799.[89] Therefore, although it is not known when exactly this third pamphlet was written, the evidence considered suggests that it was in the November/December period, which counting back the eight months referred to by Jenner, takes the origin of the vaccine back to the spring of 1799.

The nature of this main stock of lymph is indicated in a letter written by Jenner to Ring on the 18th September, 1799. Ring had written previously to Jenner asking for a supply of vaccine, to which Jenner replied:

"When I had the pleasure of receiving your letter there was no cow-pox matter here in a fit state to send you. That which is enclosed was taken about four days ago . . . This matter is from the source mentioned at the conclu-

sion of my second pamphlet. It has been passing from one patient to another for upwards of six months . . ."[90]

The matter referred to by Jenner at the end of his second pamphlet was that sent to him by Woodville in February and used by him at the beginning of March, i.e., "upwards of six months" counting back from the 18th September.[91] Fresh stocks of cowpox virus did not become available between September and the end of November, because Jenner in his letter of the 27th November to De Carro replied to a request for a fresh stock of cowpox directly from the cow by stating that "I should be extremely happy to furnish you with matter immediately from the Cow, but in this part of our Island I have not heard of the existence of the disease among Cattle for several months past . . ."[92] The language used at the end of the passage quoted from his third pamphlet — "That which I am now employing has been in use near eight months . . . and . . . we shall not be under the necessity of seeking fresh supplies from the cow" — suggests that by the end of 1799 Woodville's lymph had become the main, if not the sole, stock of Jenner's vaccine. In the letter that Jenner wrote to Lord Egremont in December, 1799, he stated that "500 people were inoculated from this source [the Kentish Farm strain]"[93] — the past tense suggesting the ending of the use of this experimental lymph. Taken in total, the evidence on the history of Jenner's vaccine in the year 1799 strongly points to the conclusion that his main stock was derived from Woodville's lymph, and more specifically from the arm of Ann Bumpus. Jenner claimed that this main stock had not undergone any changes in the eight-month period that he had used it, but we saw earlier how he veered between dismissing the eruptions on his earlier cases as mere pimples of no account and describing them as smallpox pustules which had been eliminated through arm-to-arm passage. Whatever the logic behind his reasoning, Jenner had reached the conclusion from his experience with Woodville's lymph over an eight-month period, that in future there would be no "necessity of seeking fresh supplies from the cow". At first sight, Jenner's reliance on Woodville's lymph as the basis of his main stock of vaccine appears surprising, but it is less so when it is remembered that he had

suffered from numerous failures and severe reactions in his previous vaccinations, and that Woodville's lymph "had succeeded better than any of the vaccine matter which he had tried before." Also his very restricted experience of vaccination prior to his use of Woodville's lymph and the variolous nature of the latter, led him to become confused about the nature of cowpox inoculation and its typical effects. Like most of his contemporaries he had only a very limited understanding of the nature of infectious diseases; he explained the decline in the number of pustular cases among Woodville's patients, by arguing that:

> "The cow-pox then maintains its ground having nearly destroyed the co-operating effects of the small-pox. And this event gives strength to what I have from the commencement of my experiments imagined that the latter is a malignant variety of the former; the parental root being the cow-pox."[94]

This belief enabled him to continue to use Woodville's lymph, persuading himself that cowpox was reasserting itself over the contaminated intrusion of smallpox. The early failures and severe reactions of the vaccines used before 1799, and the anomalous and confusing results with later ones, led Jenner into a form of defensive conservatism, keeping to the use of the vaccine which had been so apparently successful. He increasingly referred in letters and publications to his "stock" of vaccine, as if it were a particularly prized possession, the quality of which was guaranteed by Jenner's own personal stamp of approval and usage.

This attitude was most clearly reflected in the writings of one of Jenner's most important early supporters, John Ring. Having been supplied by Jenner with the Woodville lymph in September, he was forced to approach Jenner twice further for new supplies, because of failures in inoculating the vaccine. In November, 1799, Jenner's friend Paytherus gave him a supply of Jenner's stock, which, as we shall see later, produced 150 smallpox pustules on one of the first five cases with which it was used and was, therefore, presumably a

further supply of the Woodville strain. Ring wrote in about 1801 that, since November, 1799, "I have been so fortunate to preserve the valuable stock; and have been able to supply practitioners in most parts of the world."[95] However, he did receive an additional strain of vaccine in May, 1800, for he described how Jenner had sent him some of the lymph derived from Tanner's experiment with the inoculation of horsepox; Ring did not indicate how extensively this was used, but merely noted that:

> "Many of those who were inoculated with matter proceeding from this stock have been repeatedly subjected to the test of variolous contagion, but in vain . . . This [vaccine] proved equally efficacious with the matter I had before employed."[96]

There is evidence to believe, however, that Jenner continued his policy of relying on his original stock, and ignoring new sources of vaccine. He had been responsible for supplying Waterhouse in Boston, America, with vaccine in the early summer of 1800, and as will be seen later in detail, this produced frequent pustular eruptions and probably in one instance, led to a smallpox epidemic. Waterhouse wrote to Jenner asking for fresh supplies, requesting matter particularly "fresh from the cow", in the belief that the vaccine first sent to him had degenerated through arm-to-arm passage. In commenting on this, Ring indicated that this was neither "necessary nor practicable"[97] and Jenner himself wrote in a letter accompanying a fresh supply, that it was from the same source as the first and "from that stock I am using among all my patients here" — and concluded — "The whole is from my original stock."[98]

One factor which only became clear with a longer period of practice of vaccination was the very limited number of cases that Jenner was personally involved with. Being primarily a country doctor, he was faced with the problem of finding enough patients for continuous arm-to-arm inoculation; he was forced to make arrangements with colleagues such as Paytherus and Ring to keep up a supply of his own stock.

Waterhouse referred to this specifically, in stating that "Jenner himself informs me that he finds it needful to give and receive assistance in order to keep up the necessary supply of fresh virus for use."[99] By the beginning of 1807, Jenner had performed less than 6,000 inoculations,[100] an average of about two a day since the beginning of 1799. With such a limited series of cases, it was impossible for him to keep more than one strain of vaccine going, given his anxiety about preserving a distinctive stock of his own, which could be defined and perceived as authentic.

In 1802, a vaccine derived from cowpox discovered outside of England was for the first time imported into the country. Ring described this event in January, 1802:

> "Dr. Sacco, of Milan, has sent to Dr. Jenner some vaccine matter, originally procured from a cow in Lombardy. Some of this matter, with which I was favoured by Dr. Jenner, has excited the genuine pustule; and in my own practice, and in that of others, is now spreading the vaccine preventative in every direction . . . These are the first instance of the production of the vaccine pustule in England, by foreign matter."[101]

Although vaccine derived from Milan did become important in other parts of the world—including among the British settlements in India[102]—it appears again to have been only used on an experimental basis in England itself. The most important evidence of Jenner's continuing reliance on the original Woodville strain in the longer term, came out of the enquiry conducted by the Royal College of Physicians' Vaccination Committee in February, 1807. Jenner replied in answer to a question about the attenuation of vaccine through arm-to-arm passage, that "he now uses Matter which was taken from the Cow in 1799; he has since taken fresh Matter from the Cow, and cannot perceive the least Difference."[103] Presumably Jenner's reference to fresh matter from the cow refers to the Kentish Town vaccine, that derived from Tanner's equine lymph (which was inoculated on to a cow) in 1800, and Sacco's vaccine from Milan. There are no further references in

the literature to other sources of vaccine until 1813; on the 23rd July of that year, Jenner wrote to James Moore, Director of the National Vaccine Establishment:

> "Mr. Melon, a surgeon of repute at Lichfield . . . has sent me some of his equine virus, which I have been using from arm-to-arm for these two months past . . ."[104]

This direct use of horse pox once again appears to have been tried by Jenner only on an experimental basis, for three years later, on the 5th March, 1816, he wrote to Moore:

> "I vaccinate the poor here weekly, and . . . to the best of my knowledge, the matter [used for vaccination] . . . was derived from a cow about sixteen years ago. If there were a real necessity for a renovation, I know not what we should do, for the precautions of the farmers with respect to their horses, have driven the cow-pox from their herds."[105]

Jenner believed that cowpox was derived from horse-pox, but the important point about this passage is that it indicates that his main stock of vaccine was still that derived from his original practice. He was, however, rather vague about the exact origin of his vaccine ("to the best of my knowledge"), and the statement that it was taken "about sixteen years ago" would indicate that it originated in 1800 — when we know from the evidence earlier discussed, that his main stock was started in 1799. Similarly, in a letter dated 26th January, 1818, Jenner stated that all the vaccine "they have now in use in America, has been passing there from arm-to-arm for nearly the fifth part of a century",[106] whereas as we shall see later, the first of the series of successful vaccination in that country did not take place until 1801. This discussion of the age of the vaccine in use arose through widespread anxiety about a very significant weakening of its potency and its power to protect against smallpox; although Jenner rejected in 1816 the growing criticism that the lymph was becoming weaker through arm-to-arm transmission, in the following year, he appears to have accepted the need for a new vaccine, for having acquired another source of equine lymph in the spring

of 1817, he sent it to the National Vaccine Establishment, to Thompson in Edinburgh, to Baron, and to the Rev. Dr. Worthington in the United States.[107] Baron stated that Jenner supplied "many of his medical friends" and that "matter from this source was, I believe, very extensively diffused."[108] This is the only solid evidence of Jenner using on any scale a vaccine other than that derived from Woodville. However, the main reliance on his original stock does not appear to have been changed by the use of this new source, for the National Vaccine Establishment continued to use Jenner's old stock many years after his death. The reports of the National Vaccine Establishment suffered, like Jenner's late accounts of his vaccine, from minor historical inaccuracy; for example, in its annual report for the year 1838 (printed 11th April, 1838), it claimed that it was still using "matter originally collected by Dr. Jenner, 38 years ago",[109] whereas the report for 1840 stated that "the principal source of our supply . . . [is] the original virus furnished by Dr. Jenner, which has now passed happily through successive generations of subjects in the course of forty-three years."[110] Presumably the latter refers to Jenner's first experiment with the inoculation of cowpox in 1796, but as we have seen, this did not become a source of vaccine for Jenner's later work. What these statements do indicate is that Jenner's original main stock of vaccine continued to be used until at least the late 1830s, a conclusion confirmed by C. R. Aikin, who had, in the first instance, worked for the Jennerian Society and later became involved with the National Vaccine Establishment. In 1840, he wrote:

> "The Jennerian Society was established in 1803, and . . . I have a strong impression, almost amounting to certainty, that application was made to Dr. Jenner to furnish the first supply of virus . . . Though it would be absurd in me to assert that it had never been changed or renewed from the cow, I can only say that I have never heard of any such change, nor ever myself had an opportunity of using virus taken directly from the animal, which, I think, I should have done had it been within my reach."[111]

CHAPTER 4

The Nature of Woodville's Lymph

In order to understand the nature of Jenner's vaccine, it is necessary to review the evidence on the use of Woodville's lymph and how its characteristics changed over time with arm-to-arm inoculation. In his first publication,[112] Woodville gave details of 459 vaccinations performed at the London Smallpox Hospital; 64.5% (296) were listed as having pustular eruptions, and this is likely to be an understatement, as some cases (like Thomas Dorset's) had an eruption of pock-like pimples which were discounted by Woodville. There were a total of 35,730 pustules, an average of just under 78 pustules per person for the total 459 people in the sample. This can be compared with statistics of pustular eruptions given by William Watson for a series of variolations conducted over thirty years previously: of 74 people variolated, 62 (83.8%) had pustular eruptions, with a total of 2,362 pustules — an average of just under 32 per person for the whole sample.[113] Although there was a higher proportion of pustular eruptions in Watson's series, Woodville's had a much higher number of average pustules. There were two main strains in Woodville's series, that derived from Jane Collingridge and a slightly later strain which, in the first instance, had been derived from the inoculation of a cow with matter taken from one of the first ten cases (James Crouch) subjected to primary vaccination.[114] Of 225 cases in the Collingridge series, 157 (69.8%) had pustules as against 102 out of 212 (48.1%) in the Crouch one. At the end of the Collingridge series, all the cases were being vaccinated with lymph which at some stage had been taken from a body pustule rather than the local site of previous injection, whereas this appears not to have been the case with the Crouch strain. The first three people inoculated with the latter, however, had 300, 105 and 350 pustules (this was with lymph taken from Professor Coleman's cow) and so although we cannot be quite sure that it was a pure smallpox vaccine, the evidence points overwhelmingly to this conclusion (the Crouch strain was probably more attenuated through arm-to-arm passage, with

fewer passes through body pustules). Woodville summarised his conclusions about the nature of cowpox inoculation at the end of his *Reports:*

> "We have been told that the Cow Pox tumour has frequently produced erysimelatous inflammation and phagedenic ulceration; but the inoculated part has not ulcerated in any of the cases which have been under my care, nor have I observed inflammation to occasion any inconvenience, except in one instance . . . But it must be acknowledged, that in several instances, the Cow Pox has proved a very serious disease. In three or four cases, out of five hundred, the patient has been in considerable danger, and one child, as I have already observed, actually died under the effects of the disease. Now, if it be admitted that, at an average, one of five hundred will die of the inoculated Cow Pox, I confess I should not be disposed to introduce this disease into the Inoculation Hospital, because out of the last five thousand cases of variolous inoculation the number of deaths has not exceeded the proportion of one in six hundred . . . One important advantage which the Cow Pox is supposed to have over the Small Pox is that the former is not a contagious disease, and not to be propagated by the effluvia of persons infected with it. This is certainly true when the disorder is confined to the inoculated part, but where it produces numerous pustules on the body the exhalations they send forth are capable of infecting others in the same manner as the Small Pox. Two instances of casual infection in this way have lately fallen under my observation . . ."[115]

The conclusion is irresistable: Woodville's first series of vaccinations were producing results nearly identical to classical Suttonian variolation — he was inoculating smallpox not cowpox virus.

However, the severity of the results of his inoculations declined with arm-to-arm passage **when virus was selected from sites of previous inoculations.** Woodville himself noted:

> "that the matter taken from the arm of a patient, in whom the disorder neither produced fever nor eruption . . . has much more commonly had the effect of exciting a milder disease than the matter of the pustules, or than that which was obtained from a patient who had the disease in a severe manner".[116]

From the detailed case histories published, it is possible to analyse the effects of injections using different sources of vaccine, and such an analysis leads to a more specific conclusion than that suggested by Woodville. The following is what emerges from a breakdown of three types of vaccine, classified according to their source of origin: (1) of 106 cases inoculated with vaccine taken from the site of injection on 19 patients with no pustular eruption, 63 had eruptions (59.4%) with an average of 94 pustules each; (2) of 61 people inoculated with vaccine taken from the pustules of 11 cases with eruptions, 57 had pustular eruptions (93.6%) with an average of 96 pustules each; (3) of 219 cases inoculated with vaccine taken from the site of a previous injection on 43 patients with pustular eruptions, 122 had eruptions (55.7%) with an average of 135 pustules each. The result that stands out from these figures is the significantly smaller proportion of pustular eruptions amongst cases inoculated with material selected from a site of previous injection, irrespective of whether the patients from which the virus was taken had pustules themselves or not. As a result of Woodville's general awareness of this result, he began to select for his inoculations "matter . . . from those only, in whom the disease proved to be very mild."[117] Although he did not explicitly state that he was taking material from the sites of previous injection, this must have become the case, for he stated to a House of Commons Committee on vaccination in March, 1802 (over three years after his initial trials) that vaccination was a non-pustular disease, i.e., should be confined to merely a vesicular reaction at the site of injection.[118] He wrote a letter to *The Medical and Physical Journal* on the 13th June, 1799 in which he summarised the position on the new results of using only material selected from "very mild" cases:

> "In my *Reports of Inoculation for the Cow-Pox,* published last month, it appears that more than one half of the patients had pustules; I have, however, observed that . . . the disease in its progress from patient to patient, has actually become much milder. For out of 310 cases of cowpox, which have been since under my care, only 39 had pustules that suppurated; viz, out of the first 100, 19 had pustules, out of the second 13, and out of the last 110, only 7 had pustules."[119]

A year later, in the summer of 1800, he confirmed this attenuation of the vaccine through arm-to-arm inoculation, stating that he then had experience of the vaccination of "about 2,000 persons" — and that in the London Smallpox Hospital, "the disease continues to occasionally produce pustules, though not more than in the proportion of three or four cases out of 100."[120] By the end of 1800, "the number of pustular cases under the vaccine inoculation in the hospital, has been even less than three or four out of a hundred,"[121] and in March, 1802, still using the same stock of vaccine, when asked by the House of Commons Committee whether vaccination produced pustular eruptions on the body, he replied:

> "I believe they never do over the whole body; I have seen in some instances a few pustules in the neighbourhood of the inoculated part, but these instances are very rare, one in five hundred."[122]

Woodville, like most of his contemporaries, significantly distorted his perception of the evidence to fit into his understanding of it; however, all accounts of the results of using his vaccine confirm that it was becoming milder-and-milder over time, and that by about 1802 it was probably the case that only local pustular eruptions occurred, and then only very rarely. (Adams did admit in 1807, that "in the Small Pox Hospital in a very few instances, small variolous pustules have appeared after the vaccine scab has formed,"[123] but this was probably due at this stage to prior natural smallpox infection.).

Woodville was faced with a considerable problem in explaining these results, for he had all along believed that he

was inoculating cowpox, and never abandoned that belief. A year or so after his first series of vaccinations, he began to argue that the occurrence of pustular eruptions was due to the "variolous atmosphere" of the London Smallpox Hospital, and that many of the people vaccinated had been inadvertently infected with natural smallpox.[124] Two factors make this explanation unacceptable: (1) very few of the later vaccinations at the Hospital did result in pustular eruptions, in spite of also being exposed to natural smallpox infection; (2) most medical practitioners using Woodville's lymph outside of the Hospital did experience pustular eruptions. In fact, ironically, Woodville himself initially attempted to rebut Jenner's argument that the "town air" had somehow contaminated the vaccine, by describing cases of pustular eruptions outside of London:

> ". . . of the cases which I have stated, several were those of patients who were inoculated eight miles distance from London; yet those patients, in the proportion of about one in five, had an eruption. And at a small village, still further from London, eighteen persons were inoculated with similar Matter, in all of whom it produced pustules."[125]

He later attempted to explain the eruption of pustules in the first example with reference to a smallpox epidemic then occurring in the area; he failed to mention the embarrassing example of the one hundred per cent eruption in the second village.[126]

Woodville also later claimed that his private practice was free of the problem of pustular eruptions and stated that other practitioners using the vaccine had had similar favourable experiences. It is difficult to see how he was able to ignore the numerous accounts which appeared in contemporary medical journals, describing frequent cases of pustules and general body eruptions. Ring expressed his own surprise when reading Woodville's claim:

> "Having conceived an early prepossession in favour of vaccine inoculation, and anxiously sought for an opportunity of putting it into practice, I was much surprised, and discouraged, by the result of the two first cases [with

Woodville's vaccine] where considerable eruptions resembling the small-pox, appeared, attended with the other characteristics of that disease . . . What was my surprise at reading this paragraph [of Woodville's, denying pustular eruptions outside of the hospital], when not only the medical and miscellaneous journals and magazines, but the weekly and daily prints, have noticed the circumstance of considerable pustular eruptions, produced by matter obtained from the Small-pox Hospital! When the pamphlets written on the Cow-pox alluded to that circumstance! and when it has long been a common topic of conversation, both in private companies, and at all the medical societies in London!"[127]

These reports were not confined to London: we shall see that virtually every area of Great Britain and each foreign country which used vaccine had a similar experience. In defence of Woodville, it might be said that with arm-to-arm inoculation of the vaccine, the pustular symptoms became more and more ambiguous, a phenomenon of some importance in understanding Jenner's own experience of Woodville's lymph, and the characteristics of his stock, particularly during the first two or three years of its use.

Woodville himself does not appear to have distributed vaccine but had a working arrangement with Pearson, whereby the former was primarily responsible for production and the latter for distribution. However, Pearson did discover a source of vaccine soon after Woodville had found cowpox in Gray's Inn Lane and in 1802, he gave the following account of the early history of his part in the development of Woodville's lymph:

". . . to . . . Dr. Woodville, information was communicated in January, 1799, that the Cowpox was epizootic in Gray's Inn Lane; and at the same time I received the agreeable intelligence that the disease was also raging in the largest stock of Cows on the New Road, near Paddington to which no one could gain admittance but myself. With vaccine matter procured from these sources, Dr. Woodville instituted the trials of the new inoculation in the

Smallpox Hospital; and I carried on mine in certain situations instead of the Smallpox [inoculation] . . ."[128]

Although Pearson claimed joint origin of the vaccine employed in the London Smallpox Hospital — elsewhere he referred to himself taking cowpox from "Mr. Willan's cows in Mary-le-bone fields" in February[129] — Woodville made no mention of the latter in his report on the first three months' experience with vaccination (he gave the exact source of all his vaccine for the first 459 cases) and it is likely that he used it along with other sources of cowpox on a purely experimental basis. Pearson later stated that he had used the New Road/Marylebone source of vaccine in his own private practice with some success, but he appears to have been forced by the scarcity of private patients, to resort to a use of Woodville's lymph:

> "The matter which had never been in the Small-pox Hospital and which I myself took from the cows at the cow stables above alluded to, **scarcely ever** afforded eruptions like the small-pox; but when I obtained matter to supply my correspondents in the country, not having enough of my own hand, but obtained it from the Small-pox Hospital, it frequently according to the reports of my correspondents, and in a few cases where I practised it myself, did produce such eruptions."[130]

Pearson sent Woodville's lymph out to more than 200 medical practitioners in the United Kingdom, with an accompanying letter dated the 12th March, 1799.[131] By this time, he seems to have been relying exclusively on Woodville for the source of his vaccine; the lymph that he had sent to Jenner in the middle of February had been from this source, and when Ring had asked him for a supply, "I was informed that what he brought with him came from Dr. Woodville; not being able to take sufficient from his own patients to supply the increasing demand."[132] Pearson later mentioned taking cowpox "from a cow in March (1799) last,"[133] but as this gave rise to pustular eruptions and contradicted his other statements about the origin of his vaccine, he was almost certainly in error in mentioning this date. At the beginning of 1800 he summarised

the history of Woodville's vaccine during the previous year:

> "In the months of January and February, 1799, the breaking out of the Cow-pock in two milk farms near London, furnished the vaccine poison which Dr. Woodville and myself disseminated, not only throughout this island, but also over a greater part of the continent, as well as to other quarters of the globe . . ."[134]

Pearson was initially very surprised at the results of using Woodville's lymph: "To my astonishment the disorder proceeded exactly as the inoculated smallpox."[135] In the letter sent to the 200 medical practitioners, he stated that "the whole amount of the constitutional illness [with vaccination] seemed to be as great as in the same number of patients in the inoculated smallpox."[136] Like Woodville, however, he soon experienced a decline in the severity of symptoms with arm-to-arm inoculation of the vaccine, and in August, 1799, published the following:

> "I must correct my statement of March last . . . Since that Report, or at least for the last four months, as far as I observed and been able to learn from others, the whole amount of the constitutional illness was not one half of the whole amount in an equal number of patients inoculated for the Smallpox . . . In my private practice, not a single case with eruptions resembling the Smallpox has occurred these last four months, and but a small proportion with any eruptions of other kinds. From my correspondents I have not had a single case of eruptions like the variolous since that of Dr. Redfearn's of Lynn [in April, 1799] . . ."[137]

With the decline of the severity of symptoms, Pearson, like Woodville and Jenner, began to deny that the vaccine that he was using gave rise to pustules and other symptoms of inoculated smallpox. He was soon forced to retract this belief because of the resurgence of virulence experienced with the vaccine used at Petworth. In the light of this experience, Pearson rewrote the history of his use of vaccine which is worth quoting at length because of the light that it sheds on both the changes that were brought about by

arm-to-arm inoculation and (as we shall see later), the nature of Jenner's stock of vaccine. In February, 1800, Pearson published the following statement:

> "In the course of my practice, the latter end of February and in March following, I distinctly recollect four cases, in which I first saw eruptions from the vaccine inoculation, resembling so much those of the small-pox, that I should not have hesitated to consider them as belonging to this disease, if I had not excited them by a different poison from the variolous. I observed, however, at that time, some appearance of these eruptions different from those which usually occur in the small-pox. Almost all these eruptions, in the stage of dessication, afforded shining, smooth, black, or reddish-brown scabs; very few of them having previously suppurated. Finding, in several instances, that the matter from the inoculated pustule of these patients produced a similar eruptive disorder . . . I, from that time, avoided using matter from the cases in which such eruptions appeared. After this precaution, no eruptive cases resembling the small-pox, but certainly eruptions, in number from a single one to about a dozen, which were large, red, hard pimples, with little or no lymph, and never with any pus occurred in probably one case out of twenty or thirty. These spots, so unlike the small-pox, gave no trouble, and were of such short duration, that when I speak of eruptions I do not include them in the account; I include those only in which the eruptions resembled the small-pox: nor do I reckon among the eruptive cases, those in which, in a few instances, a rash broke out about the 14th day after inoculation, and which was as troublesome as the **Urticaria**."[138]

These spots which Pearson and other early vaccinators, including Jenner, discounted as being of no significance, were, in fact, an ominous portent of what was to follow. Here is Pearson's account in the spring of 1800 of the origin of the vaccine which led to the Petworth outbreak of smallpox:

> "In the month of October last I inoculated a child, two years of age, with the vaccine . . . The . . . disease took

> place with the usual appearances in the inoculated part, and affected the whole constitution in the ordinary manner; but a few eruptions broke out on the second or third day, after a slight fever; they were, however, only the red large pimples aforementioned, and, of course, not at all like the small-pox. Mr. Keate carried matter from this child to Brighthelmstone, where Mr. Barrett inoculated two children, who took the disease, and from one of these Mr. Keate inoculated three children. They all had the usual fever about the eighth day, and all had a number of eruptions except one, who had only five or six . . . Matter from these patients was sent to Petworth, where Mr. Andre informs me, he inoculated with it fourteen children. They all took the disease, and had eruptions like the variolous."[139]

This experience led Pearson to conclude that "from the occurrence of such [eruptive] cases in the practice of other inoculators in the last autumn and this winter, I now think it very unreasonable to doubt any longer, that . . . the genuine vaccine poison does occasionally produce a certain variety of the cow-pock, characterised by the appearance of pustules like those of the variola."[140] He added, however, that if the precaution of avoiding inoculation with matter from eruptive was observed, "not above one case with variolous-like eruptions will be produced among two hundred instances of inoculated cow-pock" and these eruptive cases were "not more severe than the ordinary kinds of inoculated small-pox."[141]

With further attenuation of the virus through arm-to-arm inoculation, however, Pearson came to modify this view (as did all his contemporaries), so that, by 1803, he was claiming that the vaccine then used, which was "principally, if not solely, that which has been generated successively in the human animal, beginning in the first instance with the matter of the London Cows,"[142] was a pure cowpox vaccine which had been contaminated "in a few cases" only with smallpox.[143] The capacity that the original vaccinators had for self-deception was almost limitless: as early as March, 1800, Pearson was claiming that "the character of the inoculated Cowpock are

the same now, that they were in the first instance, directly from the animal"[144] — a claim which echoed an almost identical one made by Jenner about Woodville's vaccine at the end of 1799.[145] However, Pearson's much more extensive experience of this lymph — he was responsible for introducing it into the army and navy, as well as through his Vaccine Institution distributing it to virtually every country of the world[146] — meant that he was forced to acknowledge the continuation of eruptions. The report of the Vaccine-Pock Institution (which he was responsible for founding) for the years 1800, 1801 and 1802, mentioned the following kinds of eruption:

> "Where there was considerable fever, on the second or third day after this fever, in a very small proportion of cases, eruptions appeared, seldom exceeding twenty, which were either hard pimples, not unlike smallpox, but scarcely with any fluid in them . . . 2. In the summer season, especially in August, a rash often broke out on the tenth or eleventh day, alleviating the specific fever but exciting much general irritation. 3. Another kind of eruption, we were rather told of than saw, was small maculae, like the measles, or those of the scarlatina anginosa. 4. A common rash has appeared on the ninth day, with great inflammation of the inoculated part, and great discharge; and a second eruption, like millet seeds, without fever. 5. A kind of eruption now and then occurred in hot weather, five to ten days after the period of vaccine fever, which was a very irritating rash, of the urticaria kind . . ."[147]

This is a unique detailed description of the eruptions which sometimes accompanied vaccination at this time; the report claimed that in the majority of cases — 449 out of 500 — no such complications occurred[148] and it is clear that, from this evidence and that given by Woodville at about the same time and earlier discussed, the lymph had been attenuated very substantially to give mainly local symptoms characteristic of classical vaccination.

Abundant confirmation of the conclusions reached so far about Woodville's lymph is to be found through the study of contemporary medical journals. Moore, the first

Director of the National Vaccine Establishment, correctly stated in his *History of Vaccination* that "the medical journals at this time teemed with cases of pustular eruptions from vaccination."[149] Reports specifically mentioning pustular cases with lymph supplied by Woodville or Pearson in 1799 and 1800, came from Finmere near Buckingham,[150] Leeds,[151] Ketley in Shropshire,[152] Kings Lynn, Norfolk,[153] Leith in Scotland,[154] Cork in Ireland,[155] Hanover in Germany,[156] Paris,[157] and Rotterdam.[158] Additional references to pustular eruptions with vaccine used in Mortlake in Surrey,[159], Edinburgh,[160] Manchester,[161] Winslow, Bucks,[162] Chobham in Surrey,[163] Wolverhampton,[164] Montrose,[165] Lucca in Italy and the Cape of Good Hope,[166] appeared, but did not state the origin of the vaccine. We shall see later that there were similar reports from medical practitioners who used Jenner's stock. The accounts of the eruptions suggested that they varied from severe reactions in a majority of the cases, to light symptoms in only a small minority. In the first category was the report of T. M. Kelson of Sevenoaks, who wrote in May of 1800 of his experiences in the previous year:

> "Early in last Spring, Dr. Pearson . . . sent me some vaccine virus, with which I inoculated several persons; in two instances only it took effect; on a man and a little girl. The former about the ninth day became ill, and so continued for several days, when a few **pink, fiery-looking blisters** appeared on his body, and then followed a very numerous eruption, exactly like small-pox of the distinct kind; they took the usual course of variolous pustules, and the man did well. From one of those **pink vesications,** I inoculated his wife, and she had the **true cow-pox,** with only **local pustules** . . . From the woman I inoculated three children, and they all had eruptions of the **variolous kind,** and were very ill during the eruptive fever. From those I inoculated a few others, some of whom had eruptions, and some had not."[167]

A similar report was published by Dr. Redfearn of Kings Lynn: all three cases vaccinated with lymph sent by Pearson had pustules — the first had forty on the "face, hands and back", the second had an eruption "perfectly analogous to

the variolous disease" which "appeared upon the face, neck, hands and legs, extending itself over the whole surface of the body", while the third only suffered from "a few eruptive spots" which "were observed upon the face and hands, not more in number than five or six."[168] The unambiguous nature of the inoculations were brought out in the descriptions of very severe reactions of the kind mentioned, for example, by Dr. John Walker of Leeds in an unpublished letter to Lettsom:

> "I was the first in this place to commence it [vaccination] but unfortunately Dr. Pearson sent me variolous matter instead of the vaccine, and I had a case or two of acute small pox, with a very copious eruption of Pustules . . . We even inoculated from one of these Patients, and the disease excited by it continued to present the variolous Symptoms, beyond the possibility of Doubt. These patients were not influenced by, or subjected to, the Small pox atmosphere. This threw a damp on the practice [in Leeds] for nearly twelve Months . . ."[169]

However, frequent and severe pustular eruptions were not characteristic of the majority of reports. More typical was the account by the Rev. Robert Holt of Finmere, who stated that of about three hundred inoculated with Woodville's lymph, only three had pustular eruptions — one with "a small pustule two inches from the incision", and the other two with "above one hundred pustules in different parts of their bodies, which assumed precisely the appearance of that given by inoculation, except that they were smaller."[170] The Rev. Holt sent some of this lymph to the Rev. W. Finch of St. Helens in November, 1799, and in his hands it produced even milder effects. Finch described a number of minor but very important symptoms which might have escaped a less scrupulous observer; he noted that in one child "a small red spot was discovered under its left thigh", another had "one pustule upon the nape of his neck" and "a few others had one or two red spots on different parts of their body, but none of them ever suppurated" — and this was out of a total of 714 cases.[171] The number of pustules that vaccinators observed partly depended on their definition of a pustule; as we have seen, both Wood-

ville and Pearson claimed that by the year 1800, the number of pustules ranged from "not above one case . . . among two hundred" (Pearson) to "three or four cases out of one hundred" (Woodville). Most contemporaries seemed to have accepted that in order for an eruption to be characterised as pustular, it had to be associated with maturation and suppuration — otherwise the eruption was referred to as a spot or a pimple and not enumerated in the count of pustules. Also some practitioners had a similar experience to Woodville's, of a rapid decline in the number of pustules through arm-to-arm inoculation. The first reported vaccination at the Manchester Infirmary took place on the 16th April, 1799 and M. Ward, a resident surgeon, described the symptoms as follows:

> "The first patient . . had a confluent eruption; and . . . she was in so much danger, as to make me very anxious about her . . . a great number of pustules appeared on her neck, breast and arms; and . . . there is a plentiful crop on every part of her body."[172]

Other cases vaccinated at the same time also had pustules and later, at the end of 1800, W. Simmons summarised the history of vaccination in Manchester:

> "An eruption of pustules was, for some time, a pretty constant attendant on this inoculation; latterly, however, seldom any have appeared on distant parts of the body, and not more in number than three or four; the inflammation on the arm, together with pustular eruption on that part, and a slight febrile attack, have constituted all the signs of the disease."[173]

The increasingly localised nature of pustular eruptions (around the site of injection) was also Woodville's experience, and we will see this has some importance in explaining the nature of the changes to these inoculations of smallpox.

CHAPTER 5

Pustular Eruptions with Jenner's Stock of Vaccine

We have now reached the point in the book where we must consider Jenner's own experience of pustular eruptions with the vaccine he was using, as well as that of medical practitioners who were supplied by him. At the outset, it must be said that Jenner was not the most reliable witness as to the nature of his own vaccine; for example, in publishing a letter from Henry Cline on the use of his Berkeley lymph in the summer of 1798, he changed an original statement in the letter — "The ulcer was not large enough to contain a pea, therefore I have not converted it into an issue as I intended" — to — "There were no eruptions."[174] This deliberate distortion of the evidence might be justified on the grounds that Jenner was trying to bring out that his original lymph was not accompanied by pustular eruptions, but it does indicate that one must treat his claims and statements on this subject with some caution. Similarly, when he sent Ring a supply of Woodville's lymph in September, 1799, he claimed to "have seen no pustules produced by it . . . except in a single instance, (and) they did not maturate."[175] — and yet as we have seen, he admitted to Lord Egremont that he had seen pustules on his "first patients" with this vaccine.

The first general reference to eruptions occurred in a letter from Jenner to De Carro on the 27th November, 1799:

> "When the **Areola** has spread wide around the inoculated Pustule I have sometimes seen a rash upon the Patient, and sometimes several pimples, small, hard and of a reddish colour have shown themselves on different parts of the body, some of which have contained a perceptible fluid at their apex. But this appearance is very rare . . ."[176]

In the same letter, Jenner claimed that with Woodville's lymph, "The Pustules, as the disease made its progress from one patient to another soon began to decrease in number, and now they are become quite extinct, the matter producing appearances exactly similar to that newly taken from the Pock

on the Nipple of the Cow."[177] As Woodville himself described a small proportion of his cases still experiencing pustules at this time, it would appear that Jenner had reached this conclusion from his own experience of declining pustules, rather than from Woodville's published accounts.

As we have seen, in his letter to Lord Egremont in December, 1799, Jenner referred to using Woodville's lymph, seeing "a few pustules" on his "first patients" — but in his "subsequent inoculations there were none". In the same letter, he claimed to have "sometimes seen, perhaps in one case in a hundred, a few scattered pimples about the body, and sometimes rashes . . .".[178] The ambiguity of language employed by Jenner — first describing the eruptions as pustules, then referring to them as pimples — characterised his later statements on the subject. In his pamphlet written at the end of 1799, he stated that he had been supplied with some of Woodville's lymph, and that "In the first instance it produced a few pustules which did not maturate; but in subsequent cases none appeared."[179] Similarly he noted that some of his correspondents had "mentioned the appearance of Small Pox-like eruptions at the commencement of their inoculations" — but he qualified this by pointing out that "the matter was derived from the original stock at the Small Pox Hospital"[180] although from descriptions and quotes from his correspondents' letters, it would appear that he had himself been supplying them with this stock of Woodville's lymph. Yet within a month or so he was denying that he had ever experienced pustules, like those characteristic of smallpox:

> ". . . from the commencement of my inoculation with the vaccine virus to the present day, no pustules, similar to the variolous, have in any one instance appeared. I have seen rashes, and sometimes (though very rarely) I have observed a few scattered pimples about the body . . ."[181]

Five months later, he repeated this statement, with a minor qualification about pustules appearing in two cases: "I have seen pimples excited by the cow-pox, with sometimes a little fluid at their apex; and, in two instances, a vaccine pustule,

resembling that on the arm produced by inoculation; but in no instance, a small-pox-like pustule."[182]

In a letter to Waterhouse, dated the 4th March, 1801, Jenner came nearest to making an admission that eruptions were fairly common accompaniments to vaccination:

> "It is by no means uncommon to see an appearance something like tooth rash. I have seen too, though very rarely, the vaccine pustule, once upon the chin of an adult, and in two or three instances among children. But every thing of the eruptive kind I have ever seen has been too immaterial to merit notice . . ."[183]

His last statement made on the subject was to the Royal College of Physicians Vaccination Committee on the 19th February, 1807; when asked whether eruptions formed a part of the disease of vaccination, his answer was recorded as follows:

> "They do, but they are rare. They consist in minute Vesicles thinly scattered, and contain a limpid fluid in their Apex, and bear no resemblance to the Pustules of Small Pox. He has not seen more than seven such Cases, nor has he generally looked for them because he considers them insignificant. In three or four Instances, he has seen Urticaria."[184]

The tone of this statement — "nor has he generally looked for them" — reveals a defensive mentality, which perhaps conceals facts too awkward and inconvenient to be accepted and therefore observed in an impartial manner. This is certainly suggested by the evidence of other practitioners using Jenner's vaccine, but it is only possible to conclude that from Jenner's own general statements, the case about small-pox pustular eruptions is not definitely proved one way or the other.

The first person to use Jenner's stock of vaccine on any scale was Dr. Joseph Marshall of Eastington in Gloucestershire. He wrote two letters to Jenner — one in April and the other in September, 1799 — which were both published. In the

first, he described how he had inoculated 107 people with Woodville's lymph, and claimed that "in only one or two of the cases have any eruptions appeared than those around the spot where the matter was inserted, and those near the infected parts."[185] It is not clear what the nature of the eruptions around the site of injection was — this kind of sattelite eruption was thought of by Ring as characteristic of inoculated smallpox. The "one or two" cases of other pustular eruption are not at all precisely described, although in his second letter, he did indicate that he was using the term pustule to refer to an eruption which had maturated.[186] Marshall further claimed that there was no substantial difference between the Woodville and Kentish Town Farm lymph that Jenner had sent him, but he did not discuss this in any detail. This is not inconsistent with the accounts of other practitioners who had used Woodville's lymph; we have seen, Pearson claimed that "variolous-like eruptions" only occurred in about one in two hundred cases. What emerged as a critical point from our review of the literature was the existence of other kinds of eruptions, not typical of classical smallpox, but modified through arm-to-arm inoculation to such an extent as to appear like an eruption of spots rather than orthodox smallpox pustules. Woodville, Pearson and Jenner tended to discount these "pimples" in their general accounts of the results of their inoculations; it is possible that Marshall treated them in the same way.

The critical importance of the definition of a pustule emerged in the next published account of the independent use of Jenner's vaccine. Dr. J. Evans of Ketley in Shropshire included pustules which did not maturate in his account of the eruptions which followed vaccination — of sixty-eight patients, "thirty-nine had an **eruption,** but only two . . . (had) pustules (which) arrived to a state of maturation, and those imperfectly."[187] Evans published a detailed account of the effects of vaccination, with the additional interest of a comparison of the results of using lymph derived from Pearson as against that coming directly from Jenner. He wrote two letters to Woodville in September, 1799, of which the following is an extract:

". . . I was particularly careful in the choice of a lancet that

had never been used for any other purpose . . . I began to inoculate on the 11th May, and continued the practice till the end of June. The [first] vaccine virus which I [used] . . . was originally sent . . . by Dr. Pearson of St. George's Hospital. On the 8th of June I used that sent me by Dr. Jenner, and no other during the remainder of my practice. The appearance of an eruption on the first two patients surprised me greatly, as well as those subsequently inoculated, till I read your "Reports", when my mind was relieved; and after I was favoured with vaccine virus by Dr. Jenner, I was convinced, from the exact similarity of its effects, that . . . [the first] was genuine."[188]

Although Evans described a case with "many distinct pustules on different parts" of the body, and stated that the parents of the patients were "so well satisfied" with the results of the inoculation "from its similarity to the smallpox", he admitted that in "the vaccine patients . . . their indisposition in general [was] less severe."[189] An important difference was in the distribution and nature of the pustules:

> "The greatest number of pustules was generally around the part where the matter was inserted; they had the appearance that the small-pox has during the eruptive fever, and all (except what I have before mentioned) went off without arriving to a state of maturation."[190]

At the end of his second letter, Evans listed each case inoculated and the number of pustules which resulted. He unfortunately does not date the cases or indicate which were vaccinated with the lymph sent by Pearson as against that sent by Jenner. He mentioned that he had used Pearson's lymph for about four weeks, and Jenner's for just over three weeks; if we divide his sample into two (he listed his cases consecutively in the order that they were inoculated), this will give us some quantitative comparison between the two lymphs. Of the first 34 cases, 23 had pustules totalling 442 (an average of 19 pustules); of the second 34, 16 had pustules totalling 392 (an average over 24 pustules). This evidence would be more or less conclusive proof that Jenner's stock of vaccine at this time was derived from Woodville's attenuated

smallpox strain, except for one possible complication of interpretation. As well as vaccinating children with the Pearson and Jenner lymphs, Evans inoculated other members of the same family wherever he could with smallpox virus. He did this so as to be able to compare the results of the two forms of inoculation, but it obviously complicates the situation by raising the possibility that some of those vaccinated were infected by the smallpox virus of their siblings. In practice, this does not appear to have happened because Evans tells us that the:

> "vaccine Patients were sooner affected . . . and the disease [was] of much shorter duration than those inoculated with variolous virus . . . Several of the [vaccine] patients slept with others who had full crops of small-pox pustules, without being in the least degree affected."[191]

This is consistent with what we know of the shorter incubation period for more attenuated forms of a disease like smallpox, and Evans unambiguously indicates that there was no secondary infection from contact with smallpox virus. Without details of the timing of symptom development, however, and in the light of the fact of contact with inoculated smallpox cases, we must conclude that there is an element of doubt on Evans's evidence about the identical nature of Pearson's and Jenner's vaccines.

The next independent evidence on Jenner's vaccine comes from an article probably written in the autumn of 1799 by Pearson, but published in the following year. He quoted two examples of Jenner's lymph producing pustules:

> "1. In a person inoculated by Dr. Jenner, in the country, who came to town, and was under the care of Mr. Cotton, 'the eruptions bore much resemblance to the inoculated Small-pox, in number from twelve to twenty'. See Mr. Cotton's letter to Dr. Pearson. 2. . . . 'I have', says Mr. Ring, in his letter to Dr. Pearson, 'inoculated thirty patients with matter given to me by Mr. Paytherus, and to him by Dr. Jenner. One of these had about 150 pustules; these were not distinguishable from variolous ones by any diagnostics with which I am acquainted. The

matter was purulent; became perfectly opaque, and on exsication, formed a scab as large as that left by the Small-pox."[192]

As Jenner never repudiated either of these two statements, we must assume that they refer to incidents which occurred as described. There is no further reference in the literature to Mr. Cotton's patient, and as far as I am aware, the letter quoted by Pearson has never been published in full. The second incident involving Ring's patients was, however, further discussed by Ring; in a letter written on the 6th May, 1800, he stated that "in the first five cases [of inoculation with Jenner's vaccine and supplied by Mr. Paytherus], one was attended with a pustular eruption."[193] He later argued that although these "varioliform pustules could not be accounted for . . . the variolous miasmata are invisible agents; and almost continually float in the atmosphere of London."[194] It is of course possible that an isolated outbreak of smallpox inflection of unknown origin could have occurred, but this explanation becomes much less plausible in the light of a more general remark made by Ring (on the 6th May, 1800) on the subject of eruptions:

> ". . . I have seen a number of persons inoculated with supposed vaccine matter, who had eruptions in a trifling degree, both in my own practice and in that of others; but the eruptions in general were not pustulous. They rather resembled the tooth-rash than the small-pox; and were neither attended with danger nor inconvenience. In some instances a small vesicle has appeared, not altogether unlike that on the arm; but without any material inflammation."[195]

The language of this statement is virtually identical to that of Pearson in his accounts of "non-variolous" eruptions of spots and pimples; and although they appeared to Ring to be "neither attended with danger nor inconvenience", like Pearson's lymph, the vaccine which produced them was later to produce full-blown cases of smallpox which probably created a minor epidemic. Ring's admission that eruptions did occur with the lymph that he was using is important, because, as we have seen, he and Jenner after the autumn of 1799,

began to use a common stock of vaccine which was used interchangeably.

One piece of evidence that goes against the line of argument being developed in this book must now be considered. On the 24th March, 1800, Stromeyer wrote a letter giving an account of his experience of vaccination in Hanover in Germany:

> "This year we have inoculated forty persons, as well with the vaccine matter received of Dr. Pearson, as that of Dr. Jenner, all of which went properly through the disease. Betwixt the London and Gloucester vaccine matter, it appears to me, there subsists an essential difference. The London matter produces frequently an eruption of small pimples, but they disappear within a day or two at the furthest. Dr. Pearson calls these eruptions pimples. The Gloucester matter has never produced this effect here; but it frequently **occasioned ulcerations of the inoculated part, of a tedious and long duration, which the former matter never did;** on account of which I now only make use of Dr. Pearson's vaccine matter."[196]

This is strong evidence for a difference in the nature of Jenner's and Pearson's lymph, and from the descriptions given by Stromeyer, the former had all the classical characteristics of a genuine cowpox vaccine, whereas the latter gave rise to symptoms more typical of an attenuated smallpox lymph. However, there are two important points to be made about this conclusion. Firstly, it should be noted that Stromeyer rejected Jenner's lymph in favour of that from Pearson, on account of the severe ulceration of the vaccine tumour. This ironically, was one of the reasons for Jenner's preference for Woodville's lymph over those he had previously used, and it is likely to be a part of the explanation as to why he continued to use the Woodville strain as the basis of his main stock, in spite of the availability of other strains of vaccine directly derived from cowpox. The second point concerns the timing of Stromeyer's report; he had been using both the Pearson and Jenner lymphs for the first two or three months of 1800, and we know that Jenner had used the Kentish Town cowpox

vaccine at the end of 1799, and had "dispersed it among others" at about that time. In the pamphlet written by him giving his information, he indicated that he had used the Kentish Town lymph on an experimental basis, and was going to continue to use the Woodville lymph as his main stock. It is likely that the vaccine that he had sent to Stromeyer (at the end of 1799 or beginning of 1800?) was the Kentish Town stock, but that future supplies sent out by him were the Woodville lymph. The origin of the vaccine that Stromeyer obtained from Jenner is entirely hypothetical, but the above conclusion is consistent with the weight of evidence both already and about to be considered. It is also important to note, that no other account pointing to a difference between the Woodville/Pearson and Jenner lymphs has come to light — and given what we know about the characteristics of the two types of vaccine, we would expect if Jenner had widely used the Kentish Town lymph or any other vaccine directly derived from cowpox, this to have shown up in the abundant literature on the subject at this time.

On the 24th April, 1800, T. S. Gooch Esq. of Hadleigh, Suffolk, wrote to Jenner giving an account of the vaccination of 611 people with lymph sent originally by Jenner:

> "I see by your last publication you suppose it impossible for a person inoculated with the pure **uncontaminated** cowpox virus to have pustules; I beg leave to mention on that subject that we have had six people with evident pustules, from which we might have inoculated. Two of them had pustules on the eye, and four on the inoculated arm near the elbow . . . We had our virus from Mrs. Gooch's sister, Lady Rouse, who had it immediately from you."[197]

A similar proportion of a small number of pustular eruptions was experienced by Dr. R. J. Thornton, who obtained a supply of Jenner's vaccine from Ring. Initially he inoculated eight cases on October the 4th, of which only one had a "solitary pustule near the inoculated one"; of 23 later cases which were described, one had "an attempt towards forming a kind of eruption, one pustule appearing on the breast, and another on the cheek . . ."[198] Thornton sent a supply of this

vaccine to William Harrison of Ulverstone, who reported on the 1st December, 1800, that out of twenty to thirty cases inoculated, one "had three pustules on her face."[199] The number of eruptions of "pimples" and more ambiguous forms of pustules is unknown, the writers of these reports typically not discussing questions of definition. In the following year, J. Blount of Birmingham wrote to Jenner (on the 14th June, 1801) to describe the results of using a supply of the former's vaccine; of three cases discussed, two did not take,

> "and the third was taken to a distance by the mother the day after inoculation, so that I never saw the child after; but I have since heard, that the mother returned in a few days with the child, who had about fifty eruptions, of what she thought was the **Small-pox**. The disease appearing so slight, she did not think it necessary to trouble me with it . . ."[200]

It must have been common for both medical practitioners and the parents of patients, to make light of the small proportion of minor pustular eruptions that did occur with vaccination in this early period. But there is some evidence now to be considered, that taking vaccine from such an eruptive pustule could lead in some instances to a restoration of the virulence of the attenuated smallpox virus used.

It was from such a pustule that one of the most active supporters of vaccination propagated a species of vaccine which degenerated into a severe form of smallpox almost identical to that experienced at Petworth. I refer to the experience of Benjamin Waterhouse, known as the "American Jenner" because of his role in introducing vaccination into the United States. I will discuss Waterhouse's experience in some detail, because it illustrates better than any of the other examples, the variolous nature of Jenner's main stock of vaccine.

CHAPTER 6

Waterhouse's Use of Jenner's Vaccine in the United States and the Marblehead Smallpox Epidemic

At the beginning of July, 1800, Waterhouse received vaccine from Haygarth of Bath, which had been "procured from Dr. Jenner's stock by Mr. Creaser."[201] On the 8th July, he commenced his practice by inoculating seven of his family;[202] unfortunately he did not give detailed descriptions of the results of these early vaccinations but enough emerges from the literature to indicate the nature of the virus involved. The first person inoculated was one of his sons, resulting in a purely local reaction, with "no pustules on his body". From the latter's arm, Waterhouse took lymph and vaccinated a second son, three years of age — with the result of "a full maturated pustule, four inches from the place of inoculation."[203] With matter from this pustule, an infant sister and a nursemaid were vaccinated:

> "They both went through the disease with . . . symptoms . . . very similar to those of the lighter kind from inoculation for the smallpox, viz. a slight dizziness and nausea, watery eyes, chilliness, soreness of the flesh, usually called by the common people in this country, "bones-ache", a general lassitude, transient pains in the stomach, loins and head, with a disinclination to animal food and exercise . . . The striking similarity of symptoms has induced some practitioners in this country . . . to conclude, that the **kine-pox** (cow-pox) was only a variety of the smallpox."[204]

Waterhouse did not give a detailed description of the number of pustules involved in these early cases, and this was partly a function of his confusion about the correct nature of vaccination. He had taken material from a pustule other than at the site of injection, and clearly believed that eruptions were a normal part of the disease, concluding that the vaccinations of six of his family had proceeded "exactly as described by Woodville and Jenner."[205] In a letter to Ring, Waterhouse,

after describing the fully maturated pustule four inches away from the site of injection in his second child, admitted "numbers, besides his own patients, who have had a considerable crop of pustules."[206] These pustules could not be accounted for by secondary smallpox infection, for as Ring pointed out, there was no evidence of smallpox in the area when Waterhouse commenced his vaccinations.[207]

As a result of his experience, Waterhouse found himself initially in agreement with Woodville and Pearson:

> "Dr. W. and Dr. P. contended that the vaccine matter was capable of producing small-pox pustules with all their phenomena as to contagion, etc. and that the vaccine disease was attended like the small-pox with pustules all over the body. This error is conspicuous in my first publication."[208]

By the middle of November, 1800, Waterhouse began to claim (like Woodville, Jenner and Pearson) that the effects of vaccination became attenuated by arm-to-arm inoculation. Other evidence on the results of vaccination in October, 1800, seem to confirm this conclusion. In a private letter to Dr. Lyman Spalding whom he had supplied with a stock of vaccine, Waterhouse warned on the 12th October "that you must take the matter from the inoculated part . . . and never from the pustules that rarely occur"[209] (presumably by this time he had become aware of what the symptoms of vaccination should consist of). Three days previously, Spalding had written to another medical colleague, stating that of fifteen people vaccinated with the lymph supplied by Waterhouse, "one only, has the appearance of eruption."[210] In October, 1800, an anonymous author stated that "in eight instances out of ten, the whole visible effect of the inoculated Kine-Pox is confined to the neighbourhood of the incision . . ."[211] It therefore appears that by early October, pustular cases were occurring, but rather infrequently. But during the month of October, very disturbing reports began to reach Waterhouse of epidemics of smallpox in Marblehead and Beverley, near Boston, which were reputed to have been started by vaccination. Waterhouse, frightened that this would discredit both the new practice and himself,

explained at some length his own role in the affair, in a letter written on the 14th November and published in the local newspaper:

> "A report having prevailed for several days past that the kine-pox had appeared in Marblehead with symptoms of unheard of virulency and even **contagion,** I feel myself particularly bound to give the public an history of the facts which gave rise to it . . . On the 14th October, Dr. Elisha Story of Marblehead, sent to me for the kine-pox matter, which I immediately sent him. His son, who did the message, informed me that his father had imported some from London, and had inoculated his sister with it, but having waited **twelve days,** and finding no symptoms to follow, considered the matter was bad, and therefore wished some of mine. Three days after, viz. on the 17th, Dr. Story wrote to me, that he had not used the matter I sent, for his daughter had that morning broken out with the disease, which was **fifteen days** after he had inoculated her. Even at the time I was struck with the description of it. He described the symptoms as very high, 'accompanied with a great number of pustules, **small and hard in the flesh, with a small margin of inflammation** around the same.' Appearances that never occur in the Kine-Pox. About this time I inoculated the son of Dr. Drury (the other Physician of Marblehead) for the Kine-Pox, and thereby allowed his father a crop of matter for his own practice. When I heard that the Kine-Pox was raging at Marblehead and Beverley, and that some were blind with it, and that moreover proved contagious; and when I recollected the symptoms, as described by Dr. Story, my mind was strongly impressed that it was not the Kine-Pox, nor a mixture of it but the **Small-Pox.** On the 12th of the present month [November], Mr. **Josiah Story** called upon me again, saying that his father wished for some of my Kine-Pox matter, for that the Kine Pox, **or something like it,** had arisen from the matter which he had imported, but with very alarming appearances . . . But what riveted my opinion that it was the Small-Pox, was his saying **'that the matter which his father inoculated,**

was taken from the arm of a sailor, on the passage between London and Marblehead,' by one of his brothers who was on board the ship; which sailor was probably inoculated with Small Pox before he left England; or else was inoculated with the Kine-Pox, and previously caught the Small Pox . . . As to the opinion that the kine-pox will increase in virulency until it finally becomes the small-pox, I never heard nor experienced any thing that favours the notion, but quite the reverse. If we are to judge the force of the disease by the number of pustules, it certainly becomes milder as it recedes from the cow. It is well known that cold diminishes the activity of febrile poisons . . . I have never seen a case of the kine-pox any how alarming . . ."[212]

This version of the epidemic at Marblehead has never been questioned by medical historians, and this is largely because nearly all the information published about it came from Waterhouse, and he both suppressed and distorted crucial facts which were deeply embarrassing to himself and his practice of vaccination. The reader will note from the above account that there were **"no symptoms"** by twelve days in Dr. Story's daughter; if Waterhouse were correct in believing that she had inadvertently been inoculated with smallpox, she would almost certainly have had a strong local reaction well within this period. Smallpox having broken out in her on the fifteenth day without previous symptoms, she is likely to have been infected with natural smallpox (i.e. through respiratory infection). Waterhouse categorically denied that any of his own vaccinated cases had the kind of severe symptoms that could have been the source of contagion, and in his letter he admits only to vaccinating Dr. Drury's son about the same time as the infection of Dr. Story's daughter. Both these statements can be shown to be false — and they can be shown to be false by statements later made by Waterhouse himself.

On the 15th December, an anonymous author under the title of "Anti-Synopsis" wrote a letter to a local newspaper complaining about the failure of vaccination to protect against smallpox: "of 50 persons there [Marblehead] inoculated with

the kine-pox matter, procured from the most approved sources, and regularly exhibiting all the symptoms of the disease with inflammation, pustules, etc., only about one tenth were protected from the ravages of the small-pox . . ."[213] The confusion in this contemporary account is apparent: the author clearly believed that "kine-pox inoculation" was a pustular disease, but appears not to have realised that much of the smallpox amongst the vaccinated cases was probably due to the inoculation of smallpox virus. Waterhouse was concerned to defend vaccination against the charge that it failed to protect against smallpox, as well as defend his own reputation (presumably he was the "most approved sources" referred to). On the 20th December, he wrote defending vaccination and elaborating his own role in the Marblehead epidemic:

> ". . . I inoculated two, and but two inhabitants of Marblehead. One was a person whose name I have forgotten, who said he would return to me in six days, that I might see if his inoculation had **taken.** But he returned no more. As I never saw him under the disorder, I cannot be answerable for him. The other man was the son of a practitioner [Dr. Drury], a boy of about eight or nine years of age. His father brought him to my house in Cambridge. I put the thread in his arm, and gave his father a small portion of the same, but never saw nor heard any more of the boy afterwards, and of course I never knew whether he ever had the genuine **local infection,** and **specific fever,** which constitutes that pecular distemper, which I have said and still believe secures the human system from the small-pox. Yet from this boy's arm, was taken, as I am informed, all the matter, with which all the others in Marblehead were inoculated. If the matter therefore, which I used, did not give the genuine disease in the **first** instance, which happens very frequently, then all the cases that followed it must of course be **spurious,** and absolutely incapable of securing the system from an attack of small-pox . . . For about two or three weeks in the month of November the matter seemed to have undergone **a deterioration** in my hands. I inoculated several

persons four or five times over before I could communicate the disease. To three of four I never could communicate it . . . I have entertained an opinion . . . that **the kine pox matter becomes milder as it recedes from the Cow,** and that in process of time it gets worn out . . . When it is known that all the vaccine matter hitherto used in America (some received at Ipswich excepted) came from **less than two inches** of infected thread which I received from Bath, in England last June, this idea will not appear altogether absurd . . ."[214]

Waterhouse conceded that vaccination had failed to protect against smallpox but continued to argue that his own cases had nothing to do with the beginning of the epidemic as such. He was prepared to admit that the vaccine had deteriorated even in his own practice, although characteristically he implied that it was the incompetence of someone else (Dr. Drury) which had given rise to the "spurious" cases in Marblehead (two years later Waterhouse made this implicit charge explicit by accusing Drury of not following his "directions respecting the **time** of taking the matter."[215]) In this letter of the 20th December there are no details of the timing of Waterhouse's vaccination of the two Marblehead cases, and no description of their symptoms — in spite of being given "a plain and candid history" of the whole sequence of events by Drs. Story, Drury and others, in visits to Salem and Marblehead before Waterhouse's first letter of the 14th November.[216]

Two years after these events of 1800, Waterhouse published a further description of what had happened which totally contradicted his initial account. He reiterated his view of the origin of the Marblehead epidemic — the unintended inoculation of smallpox virus by Dr. Story with matter taken from the sailor's arm — but gave details of his own practice of vaccination and of the cases that he had inoculated in Marblehead, which brings into question the conventional medical historical view of these events. The first major contradiction of his earlier evidence came in a statement about his first letter to the local newspapers:

"I felt it my duty in November of the same year [1800] to acquaint the public, through the medium of the news-

paper, that the kinepock had, in many places, degenerated from its original mild character, and that this deviation appeared from the inoculator taking his matter from the pustule at **too late a period.**"[217]

The reader will note by comparing this with the quote from the published November letter that Waterhouse turned the latter upside down; whereas in the published letter he claimed that the "kine-pock" was becoming milder through arm-to-arm inoculation, two years later in 1802 he was admitting that it had become severer. The reference to taking matter at too late a period originated from an idea of Jenner's, that most spurious cases were due to the timing of taking the vaccine.[218] No mention is to be found in any of Waterhouse's newspaper letter or other writings for the year of 1800 of a warning to the public about severer cases, but on the contrary the reverse is true: he publicly denied stories that the vaccine matter had deteriorated into smallpox, and stated that the lymph had become milder to the point of becoming "worn out". Yet his 1802 publication gives the details of a vaccine that had become highly virulent to the point of becoming very dangerous to those infected with it; immediately after given an account of the Marblehead epidemic, Waterhouse confessed:

> "I wish not to conceal from the public, that about the latter end of the autumn, or beginning of the winter of 1801, I perceived that the vaccine disease had deviated from its original character, and assumed a face with which I was not acquainted. I endeavoured to account for this change of countenance by persuading myself that the virus became milder as it receded from the cow; and that it would at length become effete by passing through a given number of human subjects . . . But this notion was encumbered with several difficulties, as many cases arose in direct opposition to it; for instead of becoming milder, they were in fact severer in all their symptoms. This induced some to adopt a notion directly opposite; that the small-pox was at its origin the cow-pox . . . for when the small-pox appeared first at Marblehead, it was very

universally believed that it was the cow, or kine-pock, verging in malignity to the small-pox . . . I silently entertained another whim, that the cold weather aggravated this disease . . ."[219]

He had not only persuaded himself that the virus became milder with use, but had written two long newspaper letters attempting to convince the public at large of the same notion. In his 1802 publication, he came near also to admitting that the people of Marblehead had been right about the origin of smallpox in their town:

"All those cases, where there were violent inflammation, deap seated ulceration, eruptions, and heavy febrile symptoms, were not the true kine-pock, but a malady generated by a highly acrid, putrid matter; or in one word, poisonous matter, taken from under a scab, or from an open ulcer, long after the specific virus was annihilated. I used in three or four persons some shining, glazy looking thread, which was dispensed in great profusion in Boston and its neighbourhood, in the autumn of 1800, and it produced in every case very distressing, if not alarming symptoms; severer in most respects, than the ordinary ones in the variolous inoculation . . . I am still ignorant of the genealogy of this matter; I only know that it was not the vaccine virus, whatever might have been its origin . . . I had but few cases of this cast, but nevertheless heard loud complaints from others . . . Had these severe cases continued, I should have relinquished the practice, and advised everybody else to do likewise."[220]

In fact, Waterhouse did relinquish the practice at this time, and only recommenced it with a further supply of vaccine from Jenner in 1801. The "shining, glazy looking thread" was reminiscent of the "shining, smooth, black" scabs that Pearson had experienced with the use of Woodville's lymph — and given the common origin of Waterhouse's vaccine (Jenner's stock, originating in the arm of Ann Bumpus) and Pearson's, it is likely that they were both experiencing a similar resurgence of virulence in the attenuated smallpox virus that they were using.

Marblehead, being in the neighbourhood of Boston (Waterhouse described it as "a considerable sea-port 16 miles from Boston")[221] was presumably one of the areas in which this virulent species of vaccine was used. Waterhouse never explicitly accepted responsibility for the epidemic, although he admitted that "purulent matter . . . always fails in communicating the genuine kine pox. The matter, used at Marblehead, was purulent . . ."[222] and he himself drew a parallel between the Marblehead experience and that at Petworth and elsewhere:

> "The like occurrences took place at Geneva, and at several places in England, especially at Petworth, where the virus . . . gave a **spurious** [disease] . . . the effects formed a counterpart to the disasters at Marblehead."[223]

The "purulent matter" used at Marblehead was presumably that used by Waterhouse on the two cases inoculated by him. He gives further details of these in his later publication, which reveal even more contradictions of his initial accounts. Most importantly, the identity of the first Marblehead person to be vaccinated by Waterhouse is revealed: this "was a young gentleman, Mr. F., a particular connection of Dr. S. [Story] . . . [he] was an assistant inoculator, and in some instances nurse, during the whole time that disorder [smallpox] was passing through the town . . ."[224] This implies that he was vaccinated before the beginning of the epidemic — which would make it very likely (given what we know about the virulent nature of Waterhouse's vaccine) that he was the source of the epidemic, which was reputed to have started with Dr. Story's daughter. It is possible that Mr. F. was vaccinated by Waterhouse on the 14th October, and was the carrier of the virus that the latter had arranged to send to Story during the middle of October. That at least one of the two cases had been vaccinated before the beginning of the epidemic is tacitly admitted by Waterhouse in the statement that "previously to this sad accident [the spreading of smallpox through Marblehead], Dr. Drury had inoculated about forty persons, from the arm of his own son whom I had vaccinated . . ."[225]

Dr. Story did give some account — although unfortunately with little detail of timing and symptomology — which enables us to understand the origins of the Marblehead epidemic a bit further. On the 4th May, 1801, he wrote to the Massachusetts Medical Society, describing what he knew about the new "kine-pox inoculation":

> ". . . Early in October last my son brot me home from London some Kinepox matter as the Physician who inoculated 4 Men on board his ship informed him, of which matter I inoculated eight of my family, only one took, & broke out the 17 day and contrary to my expectation produced that fatal disease the Small pox; however, in the first instances it appeared very mild, and it was the opinion of the Medical Gentlemen who saw the Patients at that time, that it was an intermediate disease, between the kine and small pox, at least, that was their opinion then. I have never had an opportunity of inoculating for the Kine pox — therefore can say little about it. A Mr. Will[m] Fittyplace who was inoculated by Dr. Waterhouse for the Kine pox, was conversant with the Small pox in the natural way & by inoculation, being an assistant inoculator under me during the Small pox last fall . . ."[226]

This letter confirms that Story inoculated his daughter with matter taken from a sailor's arm, but it gives the additional information that in seven other members of his family it gave no reaction whatsoever, and that it did not break out in his daughter until the seventeenth day (it is not clear whether this was after the date of inoculation, or the day in the month). Without additional evidence about the timing of symptoms and the exposure of this daughter to other possible sources of smallpox virus, it is impossible to be sure how she contracted the disease. Story does reveal the name of Waterhouse's patient Mr. F — Mr. Fittyplace — and as the latter was Story's assistant (helping in the mass variolation which followed the outbreak of smallpox), it is certain that he would have been in contact with the Story family during the period of initial infection. It is not possible to say from the existing evidence

whether Story's daughter caught smallpox from Fittyplace or not, but the fact that none of the other Story children were affected by their ostensible cowpox inoculation, and that the daughter was not affected until the fifteenth (seventeenth?) day, even at the site of injection, makes it likely that she caught smallpox from Fittyplace, who in turn had been infected by Waterhouse through his contaminated inoculations. We do not know for sure that Fittyplace did get smallpox from Waterhouse, but Story's reference to the patients in the plural getting a smallpox-like disease from the ostensible kine-pox inoculations, would suggest this. It is also important to note that Story conducted no further vaccinations and that the forty or so conducted in Marblehead were carried out by Drury.

Sixty-eight people died from smallpox in Marblehead.[227] There were no other epidemic outbreaks but there were threatened epidemics. For example, at the end of October, 1800, the Rev. William Bentley of Salem, visited the neighbouring town of Beverley to witness the results of introducing the new practice of vaccination:

> "October 30 . . . Went to Beverley to see the first example of the Kine Pox in our neighbourhood. Dr. Whitney inoculated his own four children, the three eldest females. The symptoms were long and severe, with shivering, pain and fainting, and lasted six days. The eldest had few pustules, but very sore, was of a slender habit. The second daughter had it less severe in the symptoms but more pustules and very sore. The third daughter of more full habit had the symptoms very severely and was loaded especially about the face, arms and feet. The Son was very faint, and feeble, has few pustules, but great debility, just recovering. As I found the pustules, they were large and the pustulous matter more clear than commonly in the smallpox, but not essentially different. The pustules did pit, were not much inflamed at the edges, were confluent, and of bright yellow when drying away."[228]

Not surprisingly, the Whitney family became "firmly persuaded that it was the smallpox" that they were suffering from. Bentley did not explicitly state the source of the vaccine

used, but given that nearly all the lymph used in America came from Waterhouse at this time, and that he admitted sending out such virulent vaccine to the neighbourhood of Boston, it is likely that Dr. Whitney had obtained it from him. Bentley at the end of November did specifically link the severe cases with Waterhouse, and unambiguously labelled them as smallpox: "we hear that Waterhouse has written to some patients his apprehensions of some of his variolous matter."[229] Bentley goes on to describe how Dr. Little of Salem inoculated his own child and others with "kine pox", and implies that the results were similar to those at Beverley, for "Dr. Little by sending the patients inoculated in his own home, to the hospital upon the first alarm has prevented much public uneasiness, and no one yet had the Natural small pox in Salem."[230] Town Meetings were called at both Beverley and Salem to consider action to contain a possible smallpox epidemic, but although wide discretionary powers were given to the Selectmen and Health Committee, the prohibition of all further forms of inoculations appears to have been sufficient. Bentley mentioned on the 11th November that "in consequence of the imprudent management of the Cow Pox, the real Small Pox has been spread over Marblehead", and added that "there are reports that the Physician who incautiously spread the disease has been shot at with a pistol."[231] Waterhouse did not escape his share of the blame; writing in 1806 of a visit to Marblehead some years previously, he stated that "I . . . did not feel disposed to proceed **alone** to **Marblehead**, as I had heard the common people were highly exasperated, and had uttered threatening speeches against me, whom they considered as the first cause of their great calamity."[232] He eventually persuaded them of his innocence by vaccinating his coach driver with lymph received in the spring of 1801, and demonstrating the "true" character of the "kine-pox" — "They all pronounced it different from any inoculation they ever saw . . ."[233] (That this was proof of the contaminated nature of his own earlier vaccination including those in Marblehead, seems to have escaped Waterhouse.)

As a result of his experiences with this strain of vaccine, Waterhouse wrote to Jenner for a fresh supply. Ring

described Waterhouse's approach to Jenner and what ensued:

> "Dr. Waterhouse . . . sent to Dr. Jenner for a fresh supply of matter; and requested that he might have some that was as recent from the cow as possible. That it was not practicable, or necessary to procure; since it was well known to Dr. Jenner . . . that the matter suffers no degeneracy, by repeated inoculation, in the human frame. The first supply which proved successful in the hands of Dr. Waterhouse . . . was . . . from the stock of Dr. Jenner. Dr. Waterhouse is again supplied from the same stock, with matter which has not suffered the least diminution of its original virtue."[234]

Jenner himself wrote a long letter on the 4th March, 1801, to Waterhouse, anxiously trying to reassure him about the safety and authenticity of the vaccine that he had sent him:

> "By the conveyance which brings you this, you will . . . receive vaccine matter, such as I employ with my patients here . . . the virus you began to inoculate with came from my stock, and . . . with a continuation of the same, I am now, almost daily, inoculating children in the metropolis . . . it . . . is from that stock I am using among all my patients here . . . The whole is from my original stock."[235]

The resupplied vaccine was used in America with much greater success than the first supply: President Jefferson wrote an account of how he used it, along with his sons-in-law, on two hundred people, and "two or three only had from two to half a dozen pustules on the inoculated arm, and nowhere else."[236] This was the same proportion and type of pustular cases that Pearson and Woodville were achieving with the more attenuated form of their vaccine; Waterhouse had at first unwittingly selected a more virulent strain of the virus when he selected matter from a pustule other than at the site of injection — with the second supply of the same stock of vaccine, he took matter only from the primary tumour and advised those whom he supplied with vaccine to do likewise. However, even with this precaution, the new supply of vaccine still continued occasionally to produce symptoms; for example,

Dr. Shore of Petersburg wrote to Waterhouse on the 27th November, 1802:

> ". . . With the vaccine virus you sent here, our physicians commenced their inoculation early in the Spring . . . in one instance it was said to be attended with a pretty numerous eruption, and in another to have excited a violent disease, attended with fever, in a person who had undergone the small-pox."[237]

Waterhouse was of course unfortunate, like Pearson in the Petworth incident, to have previously been the victim of an unpredictable resurgence of virulence in the virus. Most vaccinators had experienced some cases with pustular eruptions, but usually these were not severe. However, the general problem of the virus reverting back to a more virulent form was by no means rare; Jenner, in a letter written in March, 1801 to Waterhouse, came near to admitting this:

> "Now I conceive that at some period of your inoculation, which may now have escaped your recollection, an imperfect pustule arose, either from some peculiarity in the constitution of your patient, or some alteration in the qualities of your matter, and that from this stock you propagated. The consequence was, that continued degeneracy you complain of in the nature of the disease. The same thing has happened to many in this country, and indeed many other parts of Europe. Now either from an idiosyncracy, or some change in the nature of the virus, a variety has sprung up in the character of the pustule. The practitioners not deeply versed in the nature of cow pox has inadvertently inoculated from this variety. He proceeds with his inoculation hoping to call back his original pustule, but alas! In vain."[238]

This is a remarkably accurate description of what had happened to Waterhouse as far as it goes; all that is missing is the fact that the "imperfect pustule" was an eruptive pustule of smallpox — and the rest of facts of the situation then fall into place. (Jenner was wrong, however, not being able to attenuate this "spurious" disease — this was achieved by Woodville and Pearson through arm-to-arm inoculation.)

Jenner's admission that Waterhouse's experience was similar to that of some practitioners in England has already been discussed, and in general terms, it is a conclusion that is substantially correct — although it was rare for the virus to reach the level of virulence that it did in the Boston area.

CHAPTER 7

Vaccination on the Continent of Europe

The experience of vaccinators outside of Britain and America appears to have been very much the same as that already discussed. Evidence on this is much more difficult to find in English libraries for obvious reasons, but reports did appear in contemporary medical journals and letters which enable us to tentatively examine the nature of vaccines used abroad. Often these reports are no more of a hint of what might be uncovered through further research; for example, Baron mentions in his biography of Jenner that the latter sent some of his vaccine to his friend John Clinch, practising in Newfoundland, and according to a report in December, 1800, there were "some untoward circumstances which occurred among those who were first vaccinated at Portugal Cove."[239] No indication is given as to what these were, but it is not hard to imagine what these might have been, but only research into local source materials would settle this question. Sometimes considerable detail is available on the symptomatic results of the use of vaccines, but it is not entirely clear whether the vaccine in question came from Jenner or Woodville/Pearson. H. M. Husson, in his review of the Continental literature of vaccination in 1801, gives detailed accounts of the different kinds of eruption experienced by European practitioners: "eruptions 'rosacées', eruptions 'miliares' and some variolous-like eruptions. The eruption 'rosacée' consists of red spots on different parts of the body."[240] Husson states that these eruptions were experienced almost universally, and he mentions by name Dr. Odier of Geneva, Dr. Aubert of Paris, Dr. Sacco of Milan, Dr. Scassi at Gènes, and Dr. De Carro at Vienna, as having experienced "spurious vaccine."[241] Reports were made by the relevant medical authorities at Geneva, Paris, Rheims and elsewhere which detailed the kinds of eruptions experienced.[242] If the English vaccinators were confused about the results of their work, one can imagine the problems that other medical practitioners had in explaining the effects of their inoculations. In some cases, Continental doctors had come to England to

learn the new techniques and methods, and had gone back home carrying a supply of smallpox virus with them under the new name of cowpox vaccine. For example, Dr. Aubert had spent some time in the London Smallpox Hospital working with Woodville, and had subsequently returned to practice in Paris; he had some difficulty in propagating the new vaccine, and Woodville spent some weeks in France inoculating the "new" disease — and as this seems to have formed the basis of the main stock of French vaccine, it is not surprising that Aubert in his report on vaccination found that "the vaccine . . . manifested on all parts of the body vesicular pustules . . ."[243] Husson does mention one instance of a supply of vaccine coming from Jenner: "Dr. Odier, who had received from Jenner some thread impregnated with vaccine obtained at Geneva spurious vaccine, in his first attempt at vaccination."[244] Odier in his own reports on vaccination, mentioned the whole range of different kinds of eruption; but appears to have experienced particularly the eruptions 'miliares': "they are very small pimples . . . never suppurating . . ."[245] Orthodox smallpox pustules were also observed, but these were explained as being the result of natural smallpox infection resulting from an epidemic.[246] However, Waterhouse claimed that "the case at Geneva, under Dr. Odier, was ours exactly"[247] and Ring, in reviewing Odier's report on his experiences, expressed surprise at some of his conclusions:

> "One opinion advanced by our author, appears rather singular. He says, 'It appears certain, that the inoculated Cow-pox is as much milder than the inoculated small-pox, as the latter is milder than the natural small-pox, at least in the three first years of life: **for after the age of three years there is no great difference between the mildness of the Cow-pox and the inoculated small-Pox**'."[248]

Ring went on to note that Odier reported "eruptions resembling the smallpox . . . in about two to three cases in every hundred; and in one or two instances, the eruption was very abundant; but it appeared evidently to be owing to the epidemic smallpox then prevailing."[249] Without a much more careful scrutiny of the original evidence it is impossible to

properly evaluate the latter interpretation, but given what we know about the nature of vaccines at this time, and Odier's general account of the severity of the disease above three-year-olds, it is likely that these smallpox pustular eruptions were a result of the vaccine itself.

Jenner later denied the charge that it was his stock of vaccine that had been responsible for Odier's experiences of pustules in Geneva,[250] although he produced no evidence one way or the other to refute this claim. One major instance of Jenner's vaccine producing pustular eruptions which he did not dispute, was the experience of Danish vaccinators. In the summer of 1801, Jenner sent vaccine via his friend Marcet to Dr. Winslow, Professor of Medicine in Copenhagen; by the 19th December of the same year, 705 vaccinations had been carried out with this supply.[251] A special committee was set up by the Government to study vaccination, and direct observations were made on 297 cases, most of which had been carried out by Winslow. The committee reported that:

> "A few vaccinated have eight or ten days after inoculation, caught the small-pox, but the committee attributes this only to a prior infection before the vaccine has taken effect."[252]

The fact that the incubation period for inoculated smallpox would fall approximately in the eight to ten days range, would make one a little suspicious of this result, and a subsequent Government report stated unambiguously that vaccination was in fact a "pustular disease". Jenner wrote to Marcet in February, 1803, and discussed his own interpretation of this finding:

> "The Copenhagen Report on the subject of Vaccination is equally satisfactory with that of other Countries. Nevertheless it strikes me that the Virus they employ there, has probably undergone some change . . . for it is said in the Report, 'those inoculated, frequently have Eruptions' . . . Similar occurrences to those in Denmark, have taken place both in this Country and many others. In Hanover the same thing took place and in a District in Scotland . . . The

> origin of this Deviation in my opinion is this — the use of the Vaccine Fluid when taken so late from a Pustule, that it was **beginning** to undergo a change which rendered it incapable of exciting the same effect precisely as if it had been taken at an early period. The truth of this theory seems to have been exemplified in many instances . . .''[253]

There is of course no reason to believe from the viewpoint of current virological and medical knowledge, that the timing of taking virus would have the significant effects postulated by Jenner (and some of his contemporaries, such as Pearson, claimed from their experience, that the timing of taking the vaccine made no difference whatsoever to the effects produced). Once again, the conclusion is inescapable: pustules resulted from the practice of early vaccination, because the vaccines employed were derived from smallpox and not cowpox.

CHAPTER 8

The Attenuation of Smallpox Virus

The question to arise out of the previous review of the historical evidence on the nature of the early vaccines is, how was the attenuation of the strain of smallpox originating in Woodville's patients achieved? Woodville's own lymph employed in the London Smallpox Hospital was not discontinued until 1836, for according to Dr. George Gregory, who was physician to the hospital in the 1830s:

> "On the 20th January, 1799, cow pock was found in Mr. Harrison's dairy, in Gray's Inn Lane, from which source, Dr. Woodville, my predecessor at the Small Pox Hospital, commenced a series of vaccinations. That same stock remained in use up to the year 1836."[254]

This prolonged use of the first vaccines appears to have been very common, as was seen with the continuation of Jenner's original stock well into the 1840's, and Woodville's lymph both in the hospital and outside of it. Bousquet stated that it was used in Paris down to 1836,[255] and Steinbrenner — his father had received vaccine from Woodville in 1802 — was still using it in 1840.[256] Similarly, De Carro mentioned in a letter to Monro in 1825, that he was still using (and he had been responsible for distributing vaccine to many parts of the world) lymph derived from British and Milanese sources in 1799.[257] These vaccines were only abandoned in the late 1830s and 1840s on the grounds that they had become so weakened by arm-to-arm passage that they were no longer giving significant protection against attacks of smallpox,[258] presumably through inadequate stimulation of the antibody response. Moore in his *History of Vaccination* claimed that pustular cases were eliminated by ensuring that "vaccine lymph alone was employed",[259] and this explanation both does and does not fit the historical evidence. Inasmuch as virtually all the early vaccines were derived from smallpox, to talk about the selection of "vaccine lymph" (i.e. vaccine derived from cowpox) does not make sense; but given that all the early vaccinators

recognised the importance of taking lymph from the tumour at the site of injection, which classically was meant to be the sole eruptive symptom of vaccination, there are reasons to believe (as we have seen, for example, with Woodville's experience) that this was of some importance.

A careful reading of the historical literature reveals, in fact, that several different variolators had been able to attenuate smallpox virus through arm-to-arm passage of the kind employed by the vaccinators. The first publication to make reference to the mild effects achieved by taking virus from a previous site of inoculation was J. Z. Holwell's book on variolation in India, published in 1767. The Indian inoculators always used "matter from the inoculated pustules of the previous year", with the result that "a few pustules generally appear round the edge of the wound . . . without a single eruption on any other part of the body."[260] The following year, Angelo Gatti's book on inoculation was published in English, and among the many shrewd and sound comments on smallpox and the practice of variolation, was a note on the question of attenuation:

> "I have long suspected, that the variolous matter became milder by inoculation; and consequently, that a repetition of the like operations would render it still more harmless, though not less efficacious. This conjecture is now become a truth, from the experiments I have tried, and those which were made in England by the most experienced inoculators."[261]

Unfortunately, as far as I am aware, Gatti never published details of his experiments, and it is not even entirely clear whether he selected virus for inoculation from the sites of previous inoculations, or whether he merely selected virus from eruptive pustules of inoculated cases (it was almost certainly the former). The English inoculators that he referred to were probably the Sutton family and their associates, who had pioneered a return to safer methods of variolation during the 1760s.

There has been some misunderstanding about the

method of inoculation adopted by the Suttons; as they attempted on financial grounds to keep their innovation a secret, it was rather difficult for contemporaries to discover the reasons for their success. All the early reports in the years 1766 and 1767 however, unanimously agreed that the Suttons at the beginning of their practice took virus early from the site of a previous inoculation,[262] but it was soon claimed that experience showed that there was no difference in effect between this virus and that selected from an eruptive pustule of a case of natural smallpox.[263] In 1768, the apologist and propagandist for the Sutton family, the Rev. R. Houlton, categorically denied that their success was the result of taking virus early from the site of a previous inoculation,[264] and quoted from a pamphlet by Dr. Giles Watts to support his case:

> "I . . . (have) seen a **great number** of persons inoculated in the new way with well concocted yellow matter, taken from the natural, as well as from the inoculated small-pox, all of whom have had the distemper full as lightly as those, who have been inoculated with crude variolous lymph (i.e., matter taken early from the site of a previous inoculation)."[265]

In 1767, Bromfield published a pamphlet summarising an experiment in which a series of patients was inoculated with virus from previous inoculated cases, and twenty children inoculated with matter of "the fourteenth in descent, from the natural sort first inoculated", had the disease in the usual way — "Some of them had the disease very mild, and others rather severe . . ."[266] Bromfield did not make it clear whether virus was taken from the **sites of previous inoculations,** or whether it was merely taken from the eruptive pustules of inoculated cases. As he seemed to have been unaware of the importance of this distinction, and did not even mention it, it is likely that virus was taken at random from inoculated cases, and therefore mainly from eruptive pustules. Although some inoculators at this time were denying that virus taken from the sites of inoculation produced milder results than that taken from elsewhere, it should be realised that these conclusions were completely impressionistic, and had not been based on

serial arm-to-arm passage of one strain of virus systematically selected from previous sites of injection.

That the question of the effects of using virus from sites of previous injection had not been settled by contemporaries, was indicated by Mudge, in his discussion in 1777 of an experiment involving this type of virus. After noting that some inoculators had explained the success of the Suttons through their use of virus from the inoculation site, Mudge warned against the latter practice on the grounds that it was not a sure mode of protecting patients against future attacks of smallpox. He described the first part of the experiment as follows:

> "Messrs. Longworthy and Arscott, surgeons, in the spring of 1776, inoculated at Plympton, a neighbouring town [to Plymouth], forty patients; of which number, thirty were injected with crude matter from the arm of a young woman [from the site of inoculation], five days after she had been inoculated with concocted matter, which did eventually produce in her a pretty smart fever, and a sufficient number of eruptions. The other ten were inoculated with matter of another kind, which I procured, in a concocted state, from a pustule of the natural smallpox. The arm of all the forty patients took the injection; and the latter ten, after the eruptive fever, had the [inoculated] smallpox in the usual way. Of the other thirty, though the injection took place, so as to inflame them considerably, and to produce a very large prominent pustule, with matter on it, in each of them, yet not one of them had eruptive fever, or a single subsequent eruption, on any part of the body . . . It is to be remarked too that the matter which was in those pustules having been used to inoculate others produced on them exactly the same appearances, unattended also with either fever or smallpox."[267]

Mudge goes on to describe how the thirty patients with just a local pustule at the site of injection were again inoculated, but this time with concocted matter taken after the eruptive fever. All thirty were infected and "had smallpox in different degrees, but in the usual way of inoculated

patients."[268] Mudge concluded that concocted matter was necessary for certain inoculated infection of patients, and that the single local pustule resulting from the use of early virus selected from sites of inoculation, did not ensure protection from future attacks of smallpox. However, Mudge did not state the period between the first and second inoculations, and if this was less than ten days, the "success" of the second inoculations is not entirely surprising. But the important point to emerge out of these experiments for the present paper, is that Mudge and his colleagues were able to produce a local pustule at the site of injection, without pustular eruptions elsewhere, and were able to propagate this attenuated form of virus through inoculation, to a further group of patients. Almost identical results were achieved by John Covey, an apothecary at Basingstoke, in a similar experiment in 1787 — of nine people inoculated with virus from a site of previous inoculation, only two had pustules other than at the site of injection.[269]

Although these experiments were producing purely local reactions through the use of virus from the site of inoculation, it is clear from earlier accounts of inoculators experimenting with what they thought was the Suttonian method of inoculation, that this was not always the case. Dimsdale explicitly stated that his experience with such virus was identical to that selected from the eruptive pustules of natural smallpox,[270] and Chandler who appears at least in his early practice to have relied almost exclusively on virus from the site of injection, obviously had pustular cases, although the proportion was unclear. Chandler claimed that his patients had:

> "without one exception gone through the disease so very slightly, as scarce ever to have been sick; not one of them had been obliged to keep within doors, or how longer than is usual than a perfect state of health; not one of them has had an equivocal eruption, though some few of them has had no other appearance than that on the punctured arm; and the fullest patient has not had a number exceeding two hundred pustules."[271]

We must therefore conclude that although Mudge and colleagues, and Covey, were producing merely a local reaction

with virus from the site of inoculation, other inoculators experienced a much more heterogeneous range of symptoms with the same kind of virus. Perhaps this should not surprise us, as the modern practice of the attenuation of viruses involves multiple serial passage. It is likely therefore, that the radical attenuation of smallpox would have been achieved only through similar multiple passage, involving the arm-to-arm inoculation of a particular strain of virus.

In 1799, Salmade published a treatise on variolation which included a discussion of the effects of using virus taken from a natural case as against an inoculated case of smallpox. He quoted an experiment by Goetz which had found that a succession of twenty inoculations using matter from a previous inoculation, appeared to have no influence on the result of the ensuing disease. Like Bromfield, however, Salmade did not indicate whether virus was taken from the site of previous inoculation or from a pustule of an inoculated case; he referred to a belief amongst some French doctors, that a succession of inoculated cases would weaken the virus to a point of nullity, to the extent that it would be without effect when used in inoculation.[272] As no details are given of the evidence on which these conclusions were based, it is impossible to evaluate their significance. Nine years later, Adams published an account of experiments at the London Smallpox Hospital, deliberately designed to transform smallpox through arm-to-arm inoculation into vaccine. He was particularly concerned to obtain a tumour at the site of injection similar to that of vaccination, and claimed great success in this objective:

> "By continuing with great caution to inoculate at the hospital from Pearl Small Pox [a mild form of smallpox], and afterwards by selecting those arms which had the most appearance of Cow Pox, we at last succeeded in procuring a succession of arms so nearly resembling the vaccine, that an universal suspicion prevailed among the parents, that they were deceived by the substitution of one for the other."[273]

It is not clear how far Adams managed to eliminate pustular eruptions in the later cases, as he was more concerned to

discuss the character of the tumour than other symptoms of the inoculations. He did, however, publish a small section of the hospital register to illustrate the nature of his results, and from this it does appear that arm-to-arm inoculation using a site of previous injection did radically attenuate the virus being used. For example, on the 14th August, 1805, William Croft was inoculated from the person suffering from the mild case of natural smallpox, and had 150 pustules; from Croft, on the 26th August, Rogers was inoculated, with a result that it "was perfectly vaccine in all its stages". On 2nd September, Mary Ann Dobins was "inoculated from Rogers" and "the arm proved vaccine in all its stages":

> "From Dobins, seven were inoculated; of these, five had no eruptions, the arms were vaccine in all the stages, and in the appearance of the scab. One had a perfectly vaccine appearance on the arm, areola, and brown scab, [but] with one hundred variolous pustules . . . The other had a vaccine arm somewhat irregular, with fever, but no pustules. From the last, were inoculated four. Of these, two had vaccine arms, perfect in all their stages, and without pustules. One had the vaccine regular, excepting that the edges sloped in such a manner, that the base was broader than the apex . . . The other had small pustules . . ."[274]

Adams did not always specify whether virus was taken from the site of a previous inoculation, and in the earlier cases, whether there were pustular eruptions or not, but as he himself stated that he was "selecting those arms which had the most appearance of Cow Pox", we can assume that he was taking material from tumours at the site of injection that most resembled vaccination. He did, however, specifically mention taking virus from eruptive pustules on particular occasions, and found, as Woodville had done before him, that this produced significantly severer results:

> "From Stevens fluid was taken from both the arm and the pustules to inoculate others. The fluid from the arm produced the vaccine vesicle, though in a few instances attended with secondary vesicles. The fluid from the

pustules produced true small pox pustular cases for three successions."[275]

The main series of trial inoculations amounted to "at least two or three hundred"[276] but Adams's work was severely hampered by the current hostility of parents to vaccination, and their demand that "their own children had unequivocal symptoms of Small Pox."[277] However, in the preface to his book, Adams stated that in the "five calendar months" that had elapsed since the beginning of the series of inoculations described, "nothing has occurred but the same uniform effects from successive inoculations."[278] His overall conclusion was that "the facts are . . . sufficient to show that we were hasty in determining that the kind of smallpox matter used for inoculation, was unimportant."[279]

After a period of initial hostility — Jenner at first implacably rejected Adams's findings — contemporaries came to accept his work; Baron, for example, stated that Adams had "succeeded in producing a benign form of variola attended with scarcely any eruption of pustules and little or no constitutional affection; this species of smallpox he considered capable of being rendered fixed and permanent."[280] In fact, Baron himself believed that smallpox could be radically attenuated through arm-to-arm passage, and quoted Jenner in support of this view:

> "After a series of inoculations with true variolous matter it has often been observed that the severity of the symptoms and the number of pustules gradually diminish till only one is to be seen, at the point of insertion . . . This fact did not escape the observation of Dr. Jenner; in reference to which he has remarked in one of his memoranda, 'Here then we see the cowpox and the smallpox acting similar parts: and that in either case the virus may steal, as it were, imperceptibly through the constitution, and give no signal of its presence'."[281]

The next attempt to transform variola into vaccine through arm-to-arm inoculation took place in France at the end of 1826 and beginning of 1827. M. Guillon, an old navy

surgeon, wrote two letters to a medical colleague, describing his work, which were abstracted in one of the French medical journals:

> "This practitioner announces, that having no vaccine virus during a very fatal small-pox epidemic, he took on the 17th of December, 1826, **some variolous matter** from a girl fifteen years of age, **on the fifth day of eruption,** and he inserted it in ten places on the arm of a healthy infant still at the breast. This inoculation, M. Guillon says, produced ten beautiful **vaccine vesicles,** with which, on the ninth day, forty-two infants were inoculated, under the eyes of the local authorities: these furnished virus for the inoculation of one hundred, who were inoculated on the 3rd of January, in the presence of the magistrates and many medical men. In a second letter, dated the 16th of January, 1827, he observes: 'The numerous vaccinations [inoculations with variolous matter] effected since my discovery, confirm more and more the perfect identity of the variolous with the vaccine virus . . . Every point in which matter is inserted proceeds well, and in their intervals in many individuals an eruption of variolous pustules takes place, and produces fever'."[282]

Guillon's work appears to have been stopped by the local magistrates on the grounds of potential hazards of spreading smallpox from such inoculations. Guillon does not seem to have had a very clear idea of how he was achieving attenuated effects; the purely local response on the infant at the breast was presumably a function of partial antibody immunity acquired from the mother, but subsequent modifications must have been the result of arm-to-arm inoculations, using the sites of previous injections as the source of virus. The key point in all the inoculations which produced attenuated forms of smallpox in the post-vaccination period, was that they were conducted in the belief that the vaccine should always be selected from the purely local site of injection. One other important point to emerge out of Guillon's account, was the eruption of secondary pustules on the arms in the areas of the sites of inoculation; as we have seen, this was a feature of

many of the more attenuated forms of inoculation made with Woodville's lymph and was characteristic of other instances of the attenuation of smallpox virus through arm-to-arm inoculation at a relatively early stage.

A more informed transformation of smallpox into vaccine was undertaken by a German surgeon, Dr. Basil Thiele of Kazan, in 1839. He showed a good understanding of the historical literature on attenuation procedures:

> "From the history of the inoculation of natural smallpox I knew, on the one hand, that the reinoculation of inoculated smallpox first tried by Professor Gatti at Pisa in 1763 makes their progress milder, and that there were examples of fifteen hundred consecutive inoculation experiments without harmful consequences . . ."[283]

What the statement on the fifteen hundred consecutive inoculation experiments referred to is unclear; I have not been able to trace in Gatti's published writings an account of such a series, but this may be a function of the unavailability of the relevant literature in England. Thiele went on to quote a further unfamiliar reference: "Dr. Robert in Marseilles found that if smallpox is inoculated with milk, it can cause only local pustules, and he derives vaccine from this."[284] Again, we can only assume that Robert was practising arm-to-arm inoculation, but this would have to be checked against original information if and where it could be located. Thiele described his own derivation of vaccine from variola as follows:

> ". . . my objective was crowned with complete success . . . a perfect vaccine was formed from the inoculatory matter after nine inoculated generations; if one deviated during the second and third generation from the prescribed path, it did not happen altogether rarely that smallpox broke out all over the body, which it was possible to reduce again to vaccine . . . several hundred cases [were involved] . . . Regarding the reduction of smallpox to vaccine, the lymph from smallpox must lie first of all for ten days between sheets of glass which are stuck down with wax, and then it must be thinned down with cow's

milk and inoculated like ordinary vaccine; this inoculation forms big pocks in the inoculated place, and the high temperature occurring only once with ordinary vaccination, occurs twice . . . and sometimes pocks come into being not only in the inoculated place but also in its neighbourhood, but they are always quite small . . . This procedure must be observed for ten generations, and gradually the pock becomes just like the vaccine . . . Later experiences have shown that the consecutive high temperature fails to appear already in the fifth generation, and then . . . the immediate transfer from arm to arm [without dilution with cow's milk] can take place."[285]

The procedure of diluting smallpox matter with cow's milk cannot but provoke a wry smile, but although Thiele's methods now appear to us to be amusing, he did make some very sound observations on the attenuating process. Firstly, he recognised that it was not a simple linear process, and that there were somewhat unpredictable resurgences of virulence in the virus. Secondly, he discovered that it took serial passage of at least ten generations before the virus had begun to adopt the approximate characteristics of conventional vaccine material. And thirdly, he noted that although the eruption of pocks became localised, there were satellite eruptions, at least in the initial phases of attenuation, in the area of the site of inoculation. There is no indication of whether this vaccine of Thiele's was put into regular use, but as he had derived it (along with another vaccine derived from the inoculation of smallpox on to the udders of a cow) because of dissatisfaction with the quality of conventional vaccine, it is likely that it was adopted as a standard lymph.

The last example of the use of smallpox as a direct source of vaccine has a somewhat more modern ring. The renowned French clinician, A. Trousseau, described in 1869 how he had resorted to variolation because of the unavailability of conventional vaccine, and the results of his attempts at attenuating the virus through arm-to-arm inoculation:

". . . I proceeded with a view to communicate the disease in as mild a form as possible . . . I asked myself . . .

whether by successive series of inoculations in the human subject, an equally great modification of the disease [smallpox] could be produced as had been produced in the sheep [with tag-sore], by which the eruption had been limited to a single pustule in the spot where the inoculation had been made. I tried the experiment at the Necker Hospital . . . We obtained the desired result in some children, to the extent that the mother pustule, the master pimple (**le maître bouton**), the pustule of inoculation was alone developed, and that around it there were little pustules, its satellites. If we could be sure of always attaining equally fortunate results, inoculation ought to be the rule, for then it would be attended by no risk, and its consequences would be purely beneficial . . . Unfortunately, matters did not turn out so propitiously. In some cases, I attained complete success of having only the pustule of inoculation; but in others, in which the very same virus had been employed, there were general eruptions, and, worse still, communication of small-pox to non-inoculated persons. In one case, regarding which I shall have to speak in connection with the subject of vaccine virus, the small-pox resumed all its original violence, after having passed through a succession of individuals in a series of inoculations. This result is opposed to those recorded by the inoculators, who made out that the variolous virus becomes progressively milder as the succession of transplantations proceeds."[286]

Trousseau did not make it clear what his method of attenuating his virus was, but as in the modification of tag-sore in sheep, the inoculator "took virus from one in which the symptoms were mildest,"[287] and he emphasised the importance of achieving a single pustule at the site of inoculation, it is likely that he was taking virus from the site of inoculation in the mildest of cases. One major point stands out in his account: the flaring of virulence of the virus, even after having achieved successful attenuation to the degree of producing just a local pustule. This, to some extent, echoed Thiele's experience, but the true parallel is with the resurgence of virulence with

Pearson's vaccine in Petworth, and Jenner's in Marblehead. Ironically, Pearson, Woodville, Jenner and the other early vaccinators all persisted with their strain of attenuated smallpox in the belief that it had derived from cowpox, and after two years or so, these attenuated strains appeared to have achieved complete stability (there were no reports of resurgence of virulence after the initial year or so).

CHAPTER 9

The Virology of Smallpox Attenuation

Having reviewed all the historical evidence on the attenuation of smallpox virus through arm-to-arm inoculation, we must now consider whether the findings of current laboratory research into the virology of pox viruses can account for the findings of the historical literature. This obviously raises technical questions which can only be answered by a qualified virologist, but it is possible to discuss the findings of this book at a hypothetical level. The first major point to note is that the relationship between smallpox, cowpox and current strains of vaccinia has not been settled by modern virological research. While cowpox and vaccinia are similar in their fairly wide host range and growth characteristics, in tissue culture variola is more like vaccinia than is cowpox, with regard to cytoplasmic inclusions and general serological characteristics; ceiling temperatures in the chick embryo and the nature of the lesions on the chorioallantois after three days is different for each one of the three viruses, although they share an identical morphological structure.[288] The three viruses are very closely related in their basic characteristics, but differences have made it impossible to establish any clear-cut genealogical relationship between them. The differences are sufficiently great for Herrlich and his colleagues to have questioned whether vaccinia has been derived from either smallpox or cowpox,[289] but this notion arising out of laboratory studies is so obviously contradicted by the evidence on the historical origin of the virus, that it clearly must be in error. One of the problems is that the viruses vary so significantly **within** the three categories of classification, that definite conclusions appear, at least at this stage, very difficult to reach. If laboratory research cannot solve this question, can the historical evidence? The problem here is that although there is no doubt that vaccinia was originally derived from either smallpox or cowpox, it is not clear which particular virus was involved in the origination of current strains. Almost from the beginning of the nineteenth century, there was controversy about the relationship between the various viruses:

many workers in the field claimed to have transformed smallpox into cowpox through the inoculation of the cow, but the most recent attempts to achieve this under modern laboratory conditions have failed.[290] Some virologists have therefore argued that the successful transformation of smallpox into cowpox was a function of the inadequate safeguards against secondary contamination of cows with vaccinia virus kept in the research establishments where the variolation of cows was being attempted.[291] However, many of the later workers were aware of possible contamination, and went to some lengths to guard against this possibility,[292] but more important is the large number of claimed successful transformations under conditions which would not be likely to give rise to secondary infection. An example of this is the work of Badcock; he was a chemist living in Brighton and had suffered a severe attack of smallpox at the end of 1836, a few months after having been re-vaccinated.[293] This failure of the current strain of vaccine led him, like many of his contemporaries to seek a more effective supply and in December, 1840, he began a series of experiments involving the inoculation of cows with smallpox virus. According to Copeman, Badcock performed in the following twenty-five years "more than five hundred variolous inoculations", of which only thirty-eight were successful.[294] Badcock's vaccine was used very extensively: "he supplied it to hundreds of medical practitioners, and many thousands of children are said to have been successfully vaccinated with it."[295] As he had set up a special stable for his inoculation experiments, and he was operating at a time when most supplies of vaccine had been derived from Woodville's strain of attenuated smallpox, it is difficult to see how under such conditions secondary infection with vaccinia virus derived from cowpox, could have taken place. The relatively small proportion of successful transformations — about $7\frac{1}{2}$ per cent — would mean that only very large series such as Badcock's, could expect any degree of success. The major problem with transforming variola into vaccina through cow inoculations, is that the mechanism of transformation has never been understood, and successful outcomes have very much been hit-or-miss affairs.

Similar problems have been encountered by virologists attempting to transform cowpox into strains of virus which resemble currently used types of vaccinia virus.[296] In the way that there is a considerable body of historical evidence for the use of smallpox as the source of vaccine (either directly through arm-to-arm attenuation as demonstrated in this book, or indirectly through the inoculation of cows), there is similar evidence for the use of genuine cowpox strains as the basis of vaccines. We have seen that the Kentish Town cowpox lymph and a strain of horsepox was used by Jenner and others on a restricted basis, and after about 1838 there is no doubt that Estlin, Ceely and others began to use other genuine strains of cowpox as the source of their vaccine.[297] Since that time, both cowpox and smallpox appeared to have been used; the present strain of vaccine used in England (at the Lister Institute) is said to have originated "from a Prussian soldier with smallpox in the war of 1870."[298] (Although there is no firm evidence for this.) When it was received in this country in 1907, it was, however, in the form of calf lymph well adapted to the calf, and all vaccines have been passaged in non-human hosts since the end of the nineteenth century. This means that modern virological research into the characteristics of current strains of vaccinia cannot resolve the problem raised by this book: the nature of a vaccine derived from the attenuation of smallpox, through arm-to-arm inoculation in an exclusively human host. However, the findings of contemporary virology on the attenuation and adaptation of viruses to particular organic environments obviously have a relevance to the construction of hypotheses which might be capable of explaining the historical findings.

In 1928, J. C. G. Ledingham and D. McClean published a paper which demonstrated that vaccine virus propagated in the rabbit dermis through serial passage, led to enhanced potency of the virus for the dermis, but "a loss of propagating power on scarification surfaces,"[299] i.e. virus adapted to grow in the dermis, lost its capacity to grow effectively on the skin surface. Ledingham and McClean were unable to explain this result, but it is clear from this and other work that viruses are capable

of adapting themselves to specific areas of the body, losing their capacity to effectively reproduce themselves in other areas where critical conditions for growth are absent. In the case of the attenuation of smallpox through arm-to-arm inoculation, the virus was systematically selected from the site of a previous inoculation, where it would have grown under very different conditions to those viruses in body pustules, which would have all experienced a systematic body reaction, including blood-borne transmission. In this situation, drawing an analogy from Ledingham and McClean's work, virus from sites of inoculation would have become adapted to grow at the skin surface in epithelial cells, and would lose their capacity for systematic body growth and virulence for the total organism. This process of adaptation would consist of the natural selection of mutants efficient for growing in a particular organic environment, involving the transformation of the genotype.

The above hypothesis does not specify what the critical body environmental conditions were for the attenuation of smallpox virus. Work on the pox viruses during the last twenty years or so has shown that temperature is one of the critical conditions for growth, and that there is some relationship between the virulence of a virus and the ceiling temperature at which it will grow in various cell environments. This is not a simple one-to-one relationship: although it was originally thought that ceiling temperature was a good laboratory test for discriminating between variola major and variola minor, with the former virulent virus having a higher ceiling temperature than the latter much less virulent type, and subsequent work on intermediate African strains seemed to confirm this overall relationship, very recent research has thrown into question the validity of using ceiling temperatures as a measure of the natural virulence of smallpox viruses.[300] (The whole notion of a basic distinction between variola major and variola minor has also been brought into question by recent work.[301]) Baxby, however, has found a possible relationship between the human pathogenicity of various smallpox vaccines and their capacity to grow at elevated temperatures on the chick chorioallantois.[302]

More directly relevant to the findings of this book, is the work of Kirn and Braunwald on the attenuation of vaccinia virus through the selection of cold variants in serial passage at regularly decreasing temperatures. The wild vaccinia strain WR underwent 10 transfers at 30°C, 4 transfers at 29°, one transfer at 27° and 5 transfers at 25°, representing a total of 20 transfers; an identical strain was subjected to 21 transfers in the same condition of culture on KB cells, but at a constant temperature of 37°C, to act as a control in the measurement of virulence characteristics. Kirn and Braunwald were successful at producing a cold variant, which became very much less virulent than either the original or control strain (which was unaffected by serial passage at constant temperature), and summarised their findings as follows:

> ". . . By means of transfers at regularly decreasing temperatures, a cold variant developing at 25°C has been selected . . . The cold variant has completely lost its virulence in mice by the intracerebral route, and its intradermal infectivity in rabbits is 41 times weaker than that of the wild virus . . ."[303]

Kirn and Braunwald go on to point out that similar findings have been made with the polio and encéphalmyélocardite viruses, and conclude that the phenomena of virus sensitivity to temperature might be a general one.[304] No attempt has ever been made to produce a cold variant of variola through serial passage at decreasing temperatures, but given the similarity of the vaccinia and variola viruses (the former in many cases being derived from the latter), and the finding of a general relationship between the loss of virulence and the production of cold variants, it is likely that a cold variant of smallpox with a loss of virulence is possible. Griffith has recently summarised experience with the attenuation of a number of different viruses:

> "Most of the virus strains used in the production of currently available vaccines have been recently developed by subjecting a natural virus isolate to various attenuating procedures such as repeated subculture at low temperatures and adapting it to grow profusely in tissue cells of a

type which do not readily support growth of the freshly isolated virulent organism. These procedures have been successfully applied in the development of measles, mumps and rubella vaccines."[305]

Applying these findings to smallpox vaccines, it might be hypothesised that the early vaccines were derived through arm-to-arm inoculation of smallpox (repeated subculture) at low temperature, whereas later vaccines originated from the growth of the virus (smallpox) in tissue cells which do not readily support growth (in the cow).

The historical evidence certainly points to the importance of reduced temperature in producing the attenuated effects of inoculated smallpox. Holwell noted in his account of Indian variolation that:

". . . early on the morning succeeding the operation, four collons [an earthen pot containing about two gallons] of cold water are ordered to be thrown over the patient, from the head downwards, and to be repeated every morning and evening until the fever comes on . . . [and] then to desist until the appearance of the eruptions . . . and then to pursue the cold bathing as before, through the course of the disease, and until the scabs of the pustules drop off . . . Confinement to the house is absolutely forbid, and the inoculated are ordered to be exposed to every air that blows . . ."[306]

Gatti conducted an experiment which can be interpreted as evidence in favour of the temperature/virulence hypothesis:

". . . I desired two of my patients inoculated in the hand, to hold it in cold water as often and long as possible, from the first appearance of the local eruption to that of the fever. In both cases, the fever came on; but it was hardly perceptible, and lasted but four or five hours . . . an inoculated patient, treated according to the foregoing rules, during the first period, will have hardly any fever in the next, and certainly a very slight eruption, and perhaps none at all."[307]

One of the innovations of the Sutton family in their highly

successful practice of variolation, was what was known as the "cold method"; Sydenham in the previous century had advocated the exposure of patients suffering from natural smallpox to the fresh air, and during the summer "to cool them . . . by rising and wearing very thin clothes"[308] and the Suttons took over this policy in their inoculation practice.[309] Thomas Christie, in his account of smallpox in Ceylon, claimed that the higher temperature in that Island, and the adjoining continent of India, meant that variolation in general was less successful than it was in European countries:

> ". . . the number of Inoculated Patients [in Ceylon between the 1st October, 1800 and the 30th September, 1802] amounted to 4,158, of which number 108 died, being nearly 1 in 38 . . . it is probable that under no circumstances, could the practice be more successfully conducted amongst the adults in that Island, where the thermometer is seldom so low as 70°, and in general many degrees higher. The Variolous inoculation by European Surgeons, in most parts of India, has been chiefly confined to the children of European Parents, and from every information I have been enabled to procure, it is conceived that not less than one in forty of these died. Dr. Fleming, the distinguished head of the Medical Department in Bengal, has, I am told calculated the mortality at a still higher rate, and conceives that nearly one in thirty of the children of Europeans, inoculated with Smallpox at Calcutta, died. I am aware, that it has been said, that the proportion of deaths from inoculation by the itinerant Brahmins in Bengal, has been very small indeed, and as they avail themselves of the cold seasons for their operations . . . I can readily believe that the mortality was infinitely less than in Ceylon where we cannot be said to have a cold season . . ."[310]

The level of mortality quoted by Christie was much higher than equivalent mortality rates in England at about the same time; for example, of the 3,000 people variolated at the London Smallpox Hospital in 1806, only two died.[311] The mortality rates in Ceylon and India may have been inflated by

people catching natural smallpox before being variolated, but this also happened as we saw earlier in the London Smallpox Hospital, and is not likely to be able to explain the scale of difference in the success rate of the two series of inoculations. The minimal mortality associated with itinerant Brahmin inoculators was probably not only a function of their variolating in the cold season, but also possibly as a result of their practice of cold water treatment, as well as using virus from previous sites of inoculation.

The historical evidence quoted all more or less relates to the effect of lower temperatures on the outcome of orthodox variolation, but although this has no direct bearing on the attenuating effect of arm-to-arm inoculation, there is a logical relationship at the hypothetical level. It can be shown that there is a significant difference in the average temperature of the skin and inner body areas,[312] and it is probable that the distribution of smallpox lesions on the body, is a function of temperature sensitivity of the virus.[313] Any method of further reducing skin temperature would increase the likelihood of producing cold variants by ensuring that virus was grown at the pustular stage at reduced temperatures. Virus taken from ordinary pustules in orthodox variolation would, of course, have experienced the higher temperatures during the phase of inner-body growth, but this could explain the greater severity of conventional smallpox inoculation over the more attenuated form achieved through arm-to-arm variolation. Virus in the latter procedure would, of course, be systematically selected from the sites of injection in the skin, and would therefore be consistently exposed to the lower body surface temperatures. If one thinks of the ten generations of arm-to-arm inoculation recommended by Thiele in his attenuating procedures, one comes very near to the type of serial passage described by Kirn and Braunwald in the production of the vaccinia cold variant. Although the foregoing argument is purely hypothetical, at least a part of it should be testable in the laboratory. If variola virus taken from pustules or smallpox crusts is different in its characteristics due to a partially different temperature environment, it should display a differential sensitivity

to temperature than virus taken from the blood or other inner-body areas. As far as I am aware, no systematic comparison of the temperature characteristics of the two types of virus has ever been undertaken.

What are the overall implications of the findings of this book? As virtually all forms of vaccination during the first forty years of the nineteenth century were attenuated forms of smallpox, the conventional dichotomy between unsafe, dangerous and ineffective variolation on the one hand, and medically sound, safe and effective vaccination on the other, no longer remains valid. Variolation and vaccination should be viewed as a part of a continuum, with the former being more severe in its effects than the latter. Although it might still be argued that variolation did occasionally spread smallpox through respiratory infection — and there can be no doubt about this from the evidence of the Petworth and Marblehead incidents alone — it had the advantage of conferring a much longer period of immunity against natural attacks of smallpox than did the more attenuated vaccination. Once variolation begins to be viewed as only a variation and a more severe form of vaccination, a revision of its historical importance becomes possible. It was always very much less dangerous than conventional medical historical accounts have accused it of being; it is not possible to discuss the historical evidence for this view here, but it might be appropriate to quote from Imperato's recent (1968 and 1974) account of variolation among the Songhai of Mali, to conclude the argument. According to Imperato, out of a total of 120 people variolated in 1967, none died, and only 22 developed a severe enough reaction to be thought to have had "clinical smallpox" (all these cases were "mild, characterised by a rash composed of discrete lesions"). Even these cases, Imperato believed, were likely to have been due to prior infection from natural smallpox during an epidemic. He has described the variolations as follows:

> ". . . the variolation technique used consisted of the application of vesicular fluid with either a thorn or a bird feather to a small round area of 5mm. diameter on the deltoid area of the arm or the lateral aspect of the leg just below

the knee. There was very little tissue destruction associated with this technique and the inoculum was small . . . The variolation sites did not show any signs of secondary infection . . . According to one infirmier who had rendered medical care to both villages during the epidemic, the sequence of events of the variolation reaction was not unlike that of a normal primary vaccinal reaction. He was aware of only two instances in which satellite lesions appeared around the variolation site. Unfortunately, data could not be obtained about systematic reactions; four men in the village of Lellehoi, who had variolation scars on their right deltoid areas, indistinguishable from vaccination scars, denied any systematic reactions."[314]

It is not surprising that as only 18.3 per cent of the total cases had reactions resembling clinical smallpox (and many of these probably due to natural infection), as few as 7.1 per cent of 447 informants interviewed in a survey said that they believed that the variolation reaction could cause smallpox in others (and those were young people, subjected to the belief patterns of modern medicine). Although Imperato himself implies that this reflects an ignorance of concepts of modern disease, recent work in India has shown that the infectivity of variola virus is partly a function of its virulence,[315] and therefore the very mild cases described by Imperato would probably have very low degrees of infectivity, possibly to the point of being non-infectious. This is not to say that all forms of variolation were as safe as those described above; Imperato also describes other types of variolation using other techniques, which were more dangerous to those being inoculated, and probably to those exposed to possible respiratory infection emanating from them. The safety and nature of variolation varied very significantly depending on the technique and method of inoculation adopted.

CONCLUSION

The evidence discussed in this book points to the conclusion that the bulk of the vaccine used by Jenner and his contemporaries was not derived from cowpox, but was accidentally developed from an attenuated strain of smallpox virus. It might be asked why medical workers and historians have not previously discovered this. Those who investigated the subject in any detail (such as Creighton and Crookshank) did uncover much of the relevant information, but were unable to reach appropriate conclusions because of their involvement in practical disputes about the value of vaccination and their limited scientific understanding of the subject. The heroism with which medical history has portrayed Jenner has all the hallmarks of a medical myth, although some might prefer to see it as one of Kuhn's paradigms of science. The final judgment on the status of Jenner's contribution to medicine must be left to the historians of science, but we may conclude here that his contribution to the control and eventual elimination of smallpox, was only a part — although a very important part — of a long history of smallpox prophylaxsis, which includes both variolation and vaccination, stretching over hundreds, and perhaps even thousands of years.

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INDEX

Abrasions and cuts
  sites for development of smallpox infection, 21
Adams, Dr. Joseph, 44, 89–91
Aikin, Dr. C. R., 40
Andre, Dr., 7, 50
Arscott, Dr., 87
Attenuation of viruses, 101, 102
Aubert, Dr., 80, 81

Badcock, John, 98
Baron, Dr. John, 9, 34, 40, 80, 91
Barrett, Dr., 50
Basingstoke, Hampshire, 88
Bath, Somerset, 65
Baxby, Dr. Derrick, 100
Bengal, India, 103
Bentley, Rev. William, 75, 76
Berkeley, Gloucestershire, 8, 9
Beverley, U.S.A., 66, 67, 75, 76
Birmingham, Warwickshire, 64
Blount, Dr. J., 64
Boston, U.S.A., 72, 76
Bousquet, Dr. M., 14, 84
Braunwald, Dr. J., 101, 104
Brighton, Sussex, 50, 98
Bromfield, Dr. William, 86
Buckland, Thomas, 19
Bumpus, Ann, 22, 23, 26
Butcher, Sarah, 23, 24, 26

Calcutta, India, 103
Cape of Good Hope, 52
Ceeley, Dr. R., 12, 17, 99
Ceylon, 103
Chandler, Dr. B., 88
Chobham, Surrey, 52
Christie, Dr. Thomas, 103
Clinch, Rev. Dr. John, 80
Cline, Dr. Henry
  letter to Jenner, 55
Coleman, Mr., 32
Collingridge, Jane, 23, 26
  pustules from vaccination, 24
Colour of vaccine tumour
  disagreement between Jenner and Woodville, 22
  Jane Collingridge, 23
Copeman, Dr. S. Monckton, 98
Copenhagen, Denmark, 82
Cotton, Dr.
  description of the pustular eruptions on one of Jenner's vaccinated cases, 60
Cork, Ireland, 52
Covey, Dr. John, 88
Cow Pox, 30, 97
  absence in Gloucestershire, 35, 39
  at Berkeley, Gloucestershire, 9, 10
  at Clark's Farm, Kentish Town, 32
  at Gray's Inn Lane, 15, 46, 84
  at New Road, Paddington, 46
  at Stonehouse,

Gloucestershire, 9, 10
difficult to inoculate, 12
from Lombardy, 38
inoculated by Jenner, 8
sent from North Nibley to Jenner by Robert Tanner, 30
severe strains, 15
Cow Pox Inoculation, 15
at London Smallpox Hospital, 7
caustic treatment of the vesicle, 9, 10
Ceeley's experience of local reaction, 18
eruptions from, 18
failure to take, 9–13
followed by variolation, 7
inflammation of the arm, 9, 14
Jenner's experience of local reaction, 17
mercurial ointment treatment of arm, 9, 10
roseoala, strophules, lichen arising from, 18
severe reactions to, 9, 10, 11, 13, 14
supernumery vesicles with, 18
Creaser, Dr. T., 65
Creighton, Dr. Charles, 107
Croft, William, 90
Crookshank, Dr. E. M., 107

Darke, Dr., 10
De Carro, Dr. J., 80, 84
Denmark, 82
Government report on vaccination, 82
Dimsdale, Dr. Thomas, 88
Dixon, Dr. C. W., 21
Dobins, Mary Ann, 90
Dorset, Thomas, 23, 41
pustule-like pocks from vaccination, 24
Drury, Dr., 67, 69, 70, 73
son of, 68, 69, 73

Eastington, Gloucestershire, 30
Edinburgh, Scotland, 40, 52
Egremont, Lord, 7
Equine vaccine, 39
used extensively in 1817, 40
Estlin, Dr. J. B., 12, 99
Evans, Dr. J., 58–60
letters to Woodville, 58, 59

Finch, Rev. W., 53
Finmere, Buckinghamshire, 52, 53
Fittyplace, William, 73, 74
Fleming, Dr., 103
France
origin of vaccines in, 81

Gatti, Dr. Angelo, 85, 93, 102
Geneva, Switzerland, 73, 80–82
George, Elizabeth, 23, 24, 26
Goetz, Dr., 89
Gooch, Mrs., 63
Gooch, T. S., 63
Gregory, Dr. George, 84
Griffith, Dr. A. H., 101
Guillon, Dr. M., 91, 92

Hadleigh, Suffolk, 63
Hanover, Germany, 52, 62, 82
Harrison, Mrs., 84
Harrison, William, 64
Haygarth, Dr. J., 65
Hearn, Mary, 10
Herrlich, Dr. A., 97
Hicks, Henry, 11
Hill, James
  vaccinated by Jenner with Woodville's lymph, 28
  variolous character of vaccine tumour, 28
Holt, Rev. Robert, 53
Holwell, Dr. J. Z., 85, 102
Houlton, Rev. Robert, 86
House of Commons Committee on Vaccination (1802), 43, 44
Hull, Hannah, 23
Hull, Sarah, 23
Hull, William, 23
Husson, Dr. H. M., 80, 81

Imperato, Dr. P. J., 105, 106
India
  method of variolation in, 85, 102, 103, 104
Ipswich, U.S.A., 70

Jefferson, President, 77
Jenner, Dr. Edward
  arrangement with Paytherus and Ring for exchange of vaccine, 37
  belief that cowpox derived from horsepox, 39
  belief that cowpox primary over smallpox, 36
  belief that smallpox malignant variety of cowpox, 36
  date of leaving Berkeley for London in 1799, 31
  description of pustular eruptions with vaccination, 55, 56, 57
  description of stock of vaccine in 1800, 37
  experience of disappearance of pustules with arm-to-arm inoculation, 7, 8, 55, 56
  failure to inoculate cowpox, 12
  in London during the spring of 1799, 30
  initial rejection of Adams's work on the attenuation of smallpox virus through inoculation, 91
  letter to De Carro, 33, 35
  letter to Lord Egremont, 7, 35
  letter to Moore, 39
  letter to Ring, 32–34
  letter to Waterhouse, 36
  letter to Woodville, 12
  necessity of exchanging vaccine in order to maintain supplies, 38
  number of vaccinations performed by beginning of 1807, 38
  receives vaccine from Thomas Tanner in 1800, 31
  source of main stock of

vaccine, 34, 35
supply of Woodville's lymph from the arm of Ann Bumpus, 22
unable to supply vaccine, 15
use of Woodville's lymph on twelve patients, 27
vaccine used in 1807 derived in the year 1799, 38
Jenner's stock of vaccine
description of pustular eruptions from, 58–66, 68, 69, 72, 77, 82
Jenner, Henry, 30
Jenner, Stephen
vaccinated by Jenner with Woodville's lymph, 28
eruptions resulting from vaccination, 28
Jennerian Society
source of its vaccine, 40
Jewel, Frances, 23, 26

Kazan, Russia, 93
Keate, Dr., 50
Kelson, Dr. T. M., 52
Kentish Town Farm Lymph, 35, 58, 99
Ketley, Shropshire, 52, 58–60
King's Lynn, Norfolk, 52
Kirn, Dr. A., 101, 104
Kuhn, Dr. Thomas, 107

Lancets
used for variolation and vaccination by Woodville, 19
reground before use for vaccination by Woodville, 19
Ledingham, Dr. J. C. G., 99, 10
Leeds, Yorkshire, 52, 53
Leith, Scotland, 52
Lellehoi, Mali, 106
Lichfield, Staffordshire, 39
Lister Institute, England, 99
Little, Dr., 76
London, 68
London Smallpox Hospital, 7, 41, 42, 81, 89
cowpox inoculation in, 16
pustular eruptions with vaccine in 1807, 44
treatment of smallpox in, 15
variolation in, 16, 103, 104
Longworthy, Dr., 87
Lucca, Italy, 52

Manchester, Lancashire, 52, 54
Marblehead, U.S.A., 66–76, 96, 105
Marcet, Dr., 82
Marseilles, France, 93
Marshall, Dr. Joseph, 30, 32, 33, 58
letters to Jenner, 32
Massachusetts Medical Society, 74
McClean, Dr. D., 99, 100
Melon, Dr., 39
Milan, Italy, 38, 80
Monro, Dr. A., 84
Montrose, Scotland, 52
Moore, Dr. James, 51, 84
Mortlake, Surrey, 52

Mudge, Dr. John, 87, 88
Mundy, William, 23
  eruptive pustules from vaccination, 24

National Vaccine Establishment, 39, 40, 52
  reports of 1838 and 1840, and the origin of its vaccine from Jenner, 40
Newfoundland, 80
Necker Hospital, France, 95

Odier, Dr. Louis, 80, 81, 82

Paris, France, 52, 80, 81, 84
Paytherus, Dr., 60, 61
Pearson, Dr. George, 7, 10, 15, 46, 60, 72
  account of origin of Petworth outbreak of smallpox, 49, 50
  anxiety about caustic treatment, 10
  belief that vaccination similar to variolation, 50
  contamination of vaccine with smallpox, 50
  declining severity of symptoms from vaccination, 48, 49
  experience of eruptions from vaccination with smooth, black or reddish-brown scabs, 49
  experience of pustular eruptions from vaccination, 47–49, 54
  inoculation of cowpox, 47
  letter to Jenner, 10
  supplier of Woodville's lymph, 47, 48, 51, 52
Petersburg, U.S.A., 78
Petworth, Sussex, 7, 50, 73, 95, 105
Phipps, James, 8
Pisa, Italy, 93
Plympton, Devon, 87
Portugal Cove, Newfoundland, 80

Redfearn, Dr., 48, 52, 53
Reims, France, 80
Ring, Dr. John, 21, 31, 36, 45, 58, 60, 61, 77, 81
  receives additional supply of vaccine from Jenner, 37
  supplies Jenner's vaccine to other practitioners, 37
Robert, Dr., 93
Rockhampton, Gloucestershire, 32
Rotterdam, Netherlands, 52
Rouse, Lady, 63
Royal College of Physicians Vaccination Committee (1807), 38

Sacco, Dr., 38, 80
Salem, U S.A., 70, 75, 76
Salmade, Dr. C., 89
Scassi, Dr., 80
Scotland, 82
Sevenoaks, Kent, 52
Shore, Dr., 78
Simmons, Dr. W., 54
Slade, Thomas, 23
  eruptive pustules from

vaccination, 24
Smallpox, 97
attenuation through inoculation, 85, 89–91, 93–95
attenuation through inoculation with virus taken from site of previous injection, 85, 87, 88, 90, 92
importance of temperature in achieving attenuated inoculations, 101–104
infectivity and virulence, 105
outbreak as a result of vaccination, 7, 75
secondary infection at sites of attempted vaccination, 21
severer inoculations with virus taken from body pustules than from sites of previous injections, 90
virulence and ceiling temperatures, 100
Songhai of Mali, 105
Spalding, Dr. Lyman, 66
Stanton, Mr., 11, 12
Steinbrenner, Dr. 84
Story, Dr. Elisha, 67, 70, 73, 74
daughter of, 68
Story, Josiah, 67
Stromeyer, Dr.
description of different symptoms with Pearson's and Jenner's vaccine, 62
preference for Woodville's lymph, 62
Stroud, Gloucestershire, 11, 12
Sutton, Daniel and family, 85, 86, 102, 103
Sydenham, Dr. T., 103

Tanner, Robert, 31
Tanner, Thomas, 15, 31, 32
Tarrent, James, 23
vaccinated with vaccine from body pustule, 26
Thiele, Dr. Basil, 93, 104
Thompson, Dr., 40
Thornton, Dr. Henry, 11, 12
Thornton, Dr. R. J., 63
Trousseau, Dr. A., 94, 95

Ulverstone, Cumberland, 64
United States of America, 39, 40, 70

Vaccine
ambiguous pustular symptoms, 24
calf lymph, 9
contaminated with smallpox, 7, 15, 16, 65–67, 72
creation through inoculation of cows with smallpox, 98
description of pustular eruptions from, 7, 8, 16, 17, 23, 52–54, 67, 75, 80–82
derived from horsepox, 99
dried, 9
failure to take, 9
humanized lymph, 8
Jenner loses supply of, 9
Jenner takes to London, 9
loss of potency, 39, 84

pathogenicity and ceiling temperature, 100
taken from body pustules, 26
Vaccine-Pock Institution, 51
eruptions from vaccination, 51
Vaccinia Virus, 97
cold variant, 101
relationship with cowpox, 97, 99
relationship with smallpox, 97–99
Variola major and variola minor, 100
Variolation, 3, 74, 105, 106
after attempted cowpox inoculation, 16
number of pustules from, 41, 105, 106
spread of smallpox from, 105, 106
timing of appearance of pustules, 20
Variolation after vaccination
producing non-maturating pustules, 17
producing only local effect, 17
Vienna, 80

Wachsel, Dr., 21
Walker, Dr. John of Leeds, 53
Ward, Dr. M., 54
Waterhouse, Dr. Benjamin, 65–77, 81
letter to Jenner, 37
Watson, Dr. William, 41
Watts, Dr. Giles, 86
West, Jane
suppurating pustules from vaccination, 26
Whitney, Dr., 75, 76
Willan, Dr. Robert, 21
Winslow, Buckinghamshire, 52
Winslow, Dr., 82
Wolverhampton, Staffordshire, 52
Woodville, Dr. William, 7, 10, 15, 16, 46, 81, 84
anxiety about caustic treatment, 10
assumption that vaccination a purely local disease, 18
belief that cowpox a pustular disease resembling smallpox, 26
denial that body pustular eruptions produced by his lymph, 44
experience of attenuation of vaccine through arm-to-arm inoculation, 44
experience of ulceration and inflammation of the arm with vaccination, 42
experience of pustular eruptions with vaccination, 41, 54
his explanation of pustular eruptions with vaccination, 45
increasing mildness of vaccinations, 44
selecting vaccine from mildest cases, 43
vaccinations in London Smallpox Hospital, 41

Woodville's Lymph
absence of ulceration and inflammation of the arm, 22, 27, 28
attenuated over time, 8, 41
child who died from vaccination with, 42
Collingridge strain, 23, 41
contagiousness of vaccinated cases with severe symptoms, 42
contaminated with smallpox, 19
Crouch strain, 41
description of pustular eruptions from, 8, 19, 20, 23, 44–46, 51–53, 58–60, 62, 81
distributed in Britain and abroad, 8
from different sources, but all contaminated with smallpox, 19, 20
Jenner's concern about its possible contamination, 7, 8, 30
Jenner's experience of local symptoms with, 28
sent to Jenner by Pearson, 22
severity of symptoms in the London Smallpox Hospital, 42
supplied by Jenner to other practitioners, 30
taken from different body sites, and the resulting number of pustules, 41, 43
virus from site of previous injection milder than that from other body sites, 43
Worthington, Rev. Dr., 40

APPENDIX

Derrick Baxby has written a review of the main issues raised by this book in an artical titled "Edward Jenner, William Woodville and the Origins of Vaccina Virus", *Journal of the History of Medicine and Allied Sciences*, April 1979. It is one of the most thorough, scholarly and informed comments to appear to date, but unfortunately marred by a number of fundamental factual errors of interpretation. I will quote from his article and comment in the sequence that he makes the points in question; hopefully this will further clarify the debate and take the controveray one stage further.

On page 139 Mr. Baxby states: "Dr. Razzell believes that the strains of smallpox being used for variolation were becoming significantly attenuated by arm-to-arm passage during the eighteenth century." This is not correct — I have argued that arm-to-arm passage led to attenuation only when virus was **taken from the site of a previous inoculation.** It is an important point because it bears on the interpretation of Jenner's experience with his "vaccine": most eighteenth century variolators did not select virus from sites of previous inoculation, but Jenner did because he believed "vaccination" was only a local phenomena with a reaction confined to the site of the injection. Thus my argument that Jenner was unwittingly attenuating the smallpox virus that he was unknowingly using. Baxby also uses this misunderstanding in another part of his analysis; on page 150 he argues that if Jenner's vaccine was smallpox it must have become attenuated "very rapidly and, as we have seen, such rapid alteration of smallpox virus had not happened in pre-Jennerian times." Smallpox did attenuate very rapidly when it was systematically selected from sites of previous inoculation, as has been shown in this book in the discussion of the work of Longworthy and Arscott, Covey, Adams, Guillo, Thiele and Trousseau — but this type of attenuation was in the main experimental, and was practised primarily in the nineteenth century and not the eighteenth. Pre-Jennerian inoculators did not usually select virus from previous sites of inoculation, and it is therefore not surprising (on the present argument) that radical attenuation did not take place.

On page 144 Baxby writes: "A surprising feature of Woodville's trials was the fact that no eruptions occurred when he vaccinated private patients in the security of their houses and away from the contamintated environment of the hospital. This fact suggests that Woodville's vaccine was basically cowpox, uncontaminated with smallpox, and that the eruptions in the hospital were caused by casually acquired smallpox." This is a repetition of the orthodox view of Woodville's experience — and it is surprising that Baxby should make it in view of the evidence quoted in this book against it. For example, Woodville's statement on the matter:"...of the cases which I have stated, several were those of patients who were inoculated eight miles distance from London; yet those patients, in the proportion of about one in five, had an eruption. And at a small village, still further from London, eighteen persons were inoculated with similar Matter, in all of whom it produced pustules." (Page 45 of this book). This is an important issue because it shows that the conventional explanation of the contamination of Woodville's vaccine — that it was due to the contaminated environment of the hospital in London — is incorrect. Baxby tries to use the contamination due to the hospital environment to explain Jenner's own experience with Woodville's lymph: "What we have then is not a gradual attenuation of the Bumpus vaccine, but a dramatic reduction in its ability to produce eruptions once out of the smallpox hospital." (Baxby, page 148). Not only is the explanation incorrect, but the claimed facts about the lack of eruptions in Jenner's early patients is, as we shall now see, wrong.

Baxby writes (page 150): "As there were no generalized eruptions in Jenner's and Marshall's patients, we can assume that the Bumpus vaccine was not predominantly unmodified smallpox." This is obviously an important point, and Baxby's conclusions are contradicted by Jenner himself; for example, in his letter to Lord Egremont, Jenner wrote: "In many places where the threads (Woodville's vaccine) were sent a disease like smallpox frequently appeared; yet curious to relate, the matter, after it had been used six or seven months, gave up the variolous character entirely and assumed the vaccine; the pustules declined more and more, and at length became extinct. I made a few experiments myself with this matter, and saw a few pustules on my first patients; but in my subsequent inoculations there were none." (See page 8 of the

present book). Baxby has been misled by the fact that Jenner elsewhere denied experience of pustular eruptions with Woodville's vaccine, but I think a close reading of the evidence as discussed in this book shows otherwise. Jenner was not the most reliable witness on these questions, as shown by the contradictory nature of the statements that he made on different occasions.

Finally, Mr. Baxby argues (page 150) that "there was no apparent attenuation with respect to the number of pustules" with Woodville's vaccine. How he had reached this conclusion is puzzling: not only is there the evidence of Jenner above, and the almost unanimous agreement by everyone involved in the controversy that the number of pustules declined radically, but Woodville's own statistical evidence is quite unambiguous on this. To quote Woodville again: "In my *Reports of Inoculation for the Cow-Pox*, published last month, it appears that more than one half of the patients had pustules; I have, however, observed that ... the disease in its progress from patient to patient, has actually become much milder. For out of 310 cases of cowpox, which have been since under my care, only 39 had pustules that suppurated; viz, out of the first hundred, 19 had pustules, out of the second 13, and out of the last 110, only 7 had pustules." (See page 44 of the present book). It is necessary to have to re-quote some of the evidence discussed in this book, as the misunderstanding has been fundamental in places, and it is the only way to resolve any outstanding ambiguity. There is no doubt whatsoever that Woodville's "vaccine" underwent a rapid and radical attenuation, and this was achieved through arm-to-arm passage, using sites of previous inoculations as the source of the virus inoculated.

There is a great deal of historical evidence, particularly for countries other than England, that has never been considered in the debate about the nature of early vaccines. It is my hope that the present book will generate empirical research — for example on the use of Woodville's vaccine, which was sent all over the world — that will enable the points in question to be settled decisively one way or the other.

The Protestant Ethic and the Spirit of Capitalism: a Natural Scientific Critique

Max Weber's *The Protestant Ethic and the Spirit of Capitalism* is widely recognized as one of the most outstanding contributions made by a sociologist to the understanding of the origins and development of modern capitalist society. Yet Weber himself felt towards the end of his life that his thesis had been fundamentally misunderstood. Critics such as Sombart and Brentano had mistakenly assumed that he was concerned with the impact of religious ethical teaching on the development of practical economic conduct:

> We are interested rather in something entirely different: the influence of those psychological sanctions which, originating in religious belief and the practice of religion, gave a direction to practical conduct and held the individual to it. This is, to speak frankly, the point of the whole essay, which I had not expected to find so completely overlooked.[1]

Since Weber's death the same kind of fundamental misinterpretation has repeatedly recurred: for example, two of the most important historians to comment on his work – R. H. Tawney and Kurt Samuelsson – have both assumed that it primarily concerned the ethical doctrines preached by the leaders of the Reformation,[2] rather than the

[1] 1. Max Weber, *The Protestant Ethic and the Spirit of Capitalism,* Unwin University Books, pp. 97, 197, 217, fn3. Weber also felt that he had been misrepresented on the role of ethical doctrines on usury – this had not been a part of his main argument and has been a further source of misunderstanding of his work. See Ibid, pp. 200, 201.

[2] R. H. Tawney, "Foreword" to Max Weber, *op.cit.*; Kurt Samuelsson, *Religion and Economic Action,* 1957.

psychological effects of theological ideas propounded by them.[3] Much of this misunderstanding of Weber's thesis is due to its notoriously fragmented nature: not only did he develop it in a number of sociological works other than *The Protestant Ethic and the Spirit of Capitalism* but he made some of his most important analytical statements in the rather obscure footnotes that he later attached to this work. In some respects virtually all of his writings can be seen as relevant to the thesis, which appears to have reflected certain central personal preoccupations.[4]

The major aim of this paper is to clarify the basic nature of Weber's substantive argument, and to critically evaluate its logical validity. In order to understand this basic argument, it is necessary to examine the methodological assumptions which form a concealed but important part of his analysis. The central methodological viewpoint of this paper is diametrically opposed to that adopted by Weber: whereas he rejected sociology as a natural science in favour of a definition of it as a historical cultural discipline dealing at the explanatory level in subjective meanings and values, the present work assumes that sociology is a natural science which treats social actions and behaviour as objects to be explained in a deterministic and causal manner. Weber objected to explanations made in the

[3]Weber wrote that *The Protestant Ethic* thesis was "a contribution to the understanding of the manner in which ideas became effective forces in history". Weber, *op.cit.*, p. go. Weber summarized his position about the role of ideas in historical explanation as follows: "Not ideas, but material and ideal interests, directly govern men's conduct. Yet very frequently the "world images" that have been created by "ideas" have, like switchmen, determined the tracks along which action has pushed by the dynamic of interest." H. H. Gerth & C. Wright Mills, *From Max Weber,* Routledge & Kegan Paul, 1948, p. 280.

[4] See Arthur Mitzman, *The Iron Cage: An Historical Interpretation of Max Weber,* 1970.

form of uniform or universal generalizations and was particularly averse to the application of evolutionary concepts of the kind employed in biology.

I will argue that Weber's methodology was incapable of explaining the results of his substantive work on the protestant ethic thesis, and that he was forced by the logic of his own analysis to continually resort to the evolutionary concept of rationalization. Weber's thesis, however, leads into complex areas beyond an evolutionary perspective, the most important being the psychological consequences of the process of rationalization (anxiety and guilt resulting from disenchantment). Again, it is argued that only a natural scientific, psychological, perspective can adequately account for the results of his substantive work. However, no amount of further analysis of the concepts of rationalization and disenchantment can solve the problem posed at the beginning of the protestant ethic thesis: Why did the process of rationalization occur in so many different spheres of social life in the occidental world, and not elsewhere? No attempt will be made to discuss this question in this paper, except where it has a bearing on the mode of Weber's own analysis.

The above summary can only give the most important outlines of the arguments involved, and to fully understand the issues arising out of Weber's work it is necessary to carefully consider a wide range of his methodological and substantive writings. Weber can be classified as a neo-Kantian with respect to his most fundamental methodological assumptions. Kant's distinction between the realm of "physical nature" and the realm of "individual freedom" is reflected in the following statement made by Weber:

> every single important activity and ultimately life as a whole, if it is not to be permitted to run on as an event of nature but is instead to be consciously guided, is a series of ultimate decisions through which the soul – as in Plato – chooses its own

fate, i.e. the meaning of its activity and existence.[5]

Kant distinguished the science of physics from that of ethics, with the former formulating "laws of nature" and the latter dealing with "laws of freedom".[6] This distinction was incorporated into Rickert's classification of the sciences into the "natural" and the "historical cultural" sciences – a classification accepted by Weber.[7] Although Weber was a thorough-going historical determinist,[8] the neo-Kantian distinction between the natural and historical cultural sciences had a fundamental influence on his methodological assumptions. He made a number of statements which reflected Rickert's influence in this respect:

> We can accomplish something which is never attainable in the natural sciences, namely the subjective understanding of the action of the component individuals. The natural sciences on the other hand cannot do this, being limited to the formulation of causal uniformities in objects and events and the explanation of individual facts by applying them subjective understanding is the specific characteristic of sociological knowledge.[9]

It is a commonplace in the sociological literature that Weber attempted to combine and integrate the methods of both the natural and historical cultural sciences, but, in fact, he attempted this integration only to a very limited extent. The natural scientific

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[5] Max Weber, *The Methodology of the Social Sciences,* Free Press, 1949, p. 18. For a similar distinction made by Weber – between "freedom of action" and "the processes of nature" – see Dennis Wrong (ed.), *Max Weber,* Prentice Hall, 1970, p. 111. Also J. P. Mayer, M*ax Weber and German Politics,* Faber, 1956, p. 35.

[6] T. K. Abbott (ed.), *Kant's Theory of Ethics,* Longmans, 1927, p. 1.

[7] Weber, *Methodology,* p. 135.

[8] Ibid, p. 123; W. G. Runciman, *A Critique of Max Weber's Philosophy* of *Social Science,* Cambridge University Press, 1972, p. 50.

[9] . Max Weber, *Economy and Society,* Bedminster Press, 1968, Vol. 1, p. 15.

part of Weber's methodology was his acceptance of the necessity of empirical proof as a part of an historical determinist analysis; it was at the level of *theoretical explanation,* not the empirical testing of ideas, that he adopted the non-scientific methodology of subjective understanding. The contradiction between the determinism of his empirical historicism and the voluntarism of his explanatory methodology, seems to have escaped him, and the tension between a natural scientific explanation and a subjectivist methodology was never resolved:

> the more precisely they (uniformities) are formulated from a point of view of natural science, the less they are accessible to subjective understanding. This is never the road to interpretation in terms of subjective meaning. On the contrary, both for sociology in the present, and for history, the object of cognition is the subjective meaning complex of action.[10]

The polarity between natural scientific and meaningful explanations was reflected in the assertion that "meaningfulness naturally does not coincide with laws as such, and the more general the law the less coincidence".[11] Not only did Weber emphasize this contrast but in some sense defined the aim of his own work as combating the natural scientific method, particularly when applied to the study of human affairs.[12] The reasons for Weber's hostility to the natural sciences are complex. He had a dislike of the reduction of "profound" metaphysical and religious preoccupations to questions answerable in terms of specialized technique and believed that the natural scientific attitude led to the "disenchantment of the world":

> if these natural sciences lead to anything in this way, they are apt to make the belief that there is such a thing as the "meaning"

[10] Weber, *Economy and Society,* p. 13.
[11] Weber, *Methodology,* pp. 76-7.
[12] Ibid., pp. 186-7.

of the universe die out at its very roots.[13]

It was partly for this reason that he hated "intellectualism as the worst devil"[14], although his attitude towards scientific rationality was characterized by a complex and confused ambivalence. His hostility to the natural sciences was linked to the belief that there was an inevitable quality to the development of the "iron cage" of rationality; this largely explains his fascination with the distinctive rationality of the occidental world and his constant return to the theme of rationalization in his sociological work. But although this process of rationalization might appear to be itself a uniform generalization of the type favoured by the natural sciences, Weber was concerned to combat just such an iron sense of scientific inevitability:

> When modern biology subsumed those aspects of reality which interested us *historically,* i.e. in all their concreteness, under a universally valid evolutionary principle, which at least had the appearance – but not the actuality – of embracing everything essential about the subject in a scheme of universally valid laws, this seemed to be the final twilight of all evaluative standpoints in all the sciences ... the naturalistic viewpoint in certain decisive problems has not yet been overcome.[15]

From this point of view, it might be said that it was Darwin's ghost, and not Marx's, that most haunted Weber.

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[13] Gerth & Mills, op. cit., p. 152. Weber's analysis of the "disenchantment of the world" appears to have been grounded on changes in his own personal religious beliefs. Mayer, op cit., pp. 24, 25, 117. As a result of the loss of "meaningfulness" associated with religious faith, "the intellectual seeks in various ways, the casuistry of which extends into infinity, to endow his life with a pervasive meaning, and thus to find unity with himself, with his fellow men, and with the cosmos". Max Weber, *The Sociology of Religion,* Methuen, 1965, pp. 124, 125.

[14] Gerth & Mills, op. cit., p. 152.

[15] Weber, *Methodology,* pp. 86, 87.

The above passage indicates Weber's own interest in the study of history: "the understanding of the characteristic uniqueness of the reality in which we move".[16] The historical cultural sciences were primarily interested in the unique and concrete flow of particular historical events. Analytical uniformities and generalizations might be occasionally useful as heuristic devices for understanding historical reality but this was rarely the case as "the specific meaning which a phenomenon has for us is naturally *not* to be found in those relationships which it shares with many other phenomena".[17] It is for this reason that the ideal types employed by Weber are not analytical concepts but are "ideal" categories used for understanding the concrete motives of individuals in the actual historical process. This emphasis on individual action explains the sociological testament written by Weber towards the end of his life:

> If I have become a sociologist (according to my letter of accreditation) it is mainly to exorcise the spectre of collective conceptions which still lingers among us. In other words, sociology itself can only proceed from the actions of one or more separate individuals and must therefore adopt strictly individualistic methods.[18]

One of the most important of these individualistic methods is of course the ideal type. In order to understand Weber's use of this much abused term, it is necessary to see it not only in terms of his individualism but also his "idealistic" concern for subjective meanings and value commitments. His problem was the construction of conceptual tools and methodological assumptions which would allow him to undertake an analysis of social meanings and cultural values

[16] Ibid., p. 72.
[17] Ibid., pp. 76, 77.
[18] Wolfgang Mommsen, "Max Weber's Political Sociology and his Philosophy of World History", *International Social Science Journal,* vol. xvii (1965), p. 44, fn 2.

"logically in exactly the same way as causal analysis of personal actions".[19] In this idealistic formula, Weber is attempting to bridge the gap between individual actions and social values, but we shall see there are good logical reasons why he failed in this. It is not possible here to discuss Weber's rather tortuous and confused analysis of ideal types but we may note the difficulty he had in constructing this conceptual bridge. He was forced to resort to metaphysical language to attempt to resolve this problem; e.g. in discussing ideal-typical analysis of political structures he wrote:

> I am making it explicit to myself and others in an *interpretative* way the concrete, individual, and on that account, in the last analysis, unique form in which "ideas" – to employ for once a metaphysical usage – are "incorporated" into or "work themselves out" in the political structure in question ... .[20]

This resort to metaphysical language was in spite of an explicit rejection elsewhere of metaphysical notions such as a "group mind" and the "Hegelian idea" from which the individual components "emanate".[21] Although Weber rejected such philosophical idealism, in practice he smuggled some of its assumptions back into his work through constructs like the ideal-type – and in this respect he was a methodological rather than a philosophical idealist.

It was on the basis of these methodological assumptions that Weber undertook to explain the process of historical change in terms of the motivations of individuals, so that for example when he discussed the origin of socialist communities, he formulated the problem as follows:

> The real empirical sociological investigation begins with the question: What motives determine and lead the individual members and participants in this socialistic community to behave in such a way that the community came into being in the first place

[19] Weber, *Methodology,* p. 177.
[20] Ibid, p. 157.
[21] Weber, *Economy and Society,* p. xxxviii.

and that it continues to exist?[22]

The central logical difficulty of a sociological explanation made in terms of these methodological assumptions – what Parsons has called a voluntaristic theory of social action – was pointed out by Durkheim in his *Rules of Sociological Method*:

> Where purpose reigns, there reigns also a more or less wide contingency; for there are no ends, and even fewer means, which necessarily control all men ... If, then, it were true that historic development took place in terms of ends clearly or obscurely felt, social facts should present the most infinite diversity; and all comparison should almost be impossible.[23]

Of course where ends and values are brought about by social or biological forces (environment and heredity) social facts can be the result of purposive choices, but such choices simply become intermediary psychological processes between one social (or biological) fact and another. It is for this reason that Durkheim insisted that one social fact must be explained by another social fact, although he has other reasons for invoking the social which border on the metaphysical. In principle there is no logical reason why a social fact cannot be derived from a biological one, but given the fundamental biological similarity of human beings in all societies, the only social facts to be explained by biological factors must necessarily be universally applicable to all social situations. (Perhaps an example of this type is to be found in universal differences in social role between the sexes – although there are some sociologists who would dispute the assumption that these differences are due to biological distinctions.) Whatever the role of biological factors in universal cultural facts, it is indisputable that societal *variations* cannot be explained by an unchanging *constant*

[22] Ibid., p. 18.
[23] Emile Durkheim, *The Rules of Sociological Method,* Free Press Paperback, 1964, p. 94.

factor such as man's bio- logical nature (this assumes that there are no significant biological variations from one society to another). Similarly, voluntaristic choices made by individuals uninfluenced by environmental factors must necessarily result in a set of randomized personal aims. The most appropriate image to convey this effect is the statistician's scatter diagram: plot a number of individual points unrelated to each other and the result will be the absence of any focus or trend in the distribution of the points – in sociological terms an absence of a social fact involving shared expectations and social meanings.

Weber himself appears at times to have been aware of this logical difficulty in any voluntaristic theory of the origin of social factors. For example in *The Protestant Ethic* he wrote that

> In order that a manner of life so well adapted to the peculiarities of capitalism could be selected at all, i.e. should come to dominate others, it had to originate somewhere, and not in isolated individuals alone, but as a way of life common to whole groups of men.[24]

But it was at this point of trying to explain the origin of "a way of life common to whole groups of men" that Weber had the greatest difficulty. With some perplexity he stated at the beginning of *The Protestant Ethic:*

> When we find again and again that, even in departments of life apparently mutually independent certain types of rationalization have developed in the Occident, and only there, it would be natural to suspect that the most important reason lay in differences of heredity. The author admits that he is inclined to think the importance of biological heredity very great. But ... it must be one of the tasks of sociological and historical investigation first to analyse all the influences and causal relationships which can be satisfactorily explained in terms of reaction to environmental

[24] Weber, *Protestant Ethic,* p. 55.

conditions.[25]

Elsewhere, Weber speculated on the possibility that "there are typical relations between certain kinds of rationality and the cephalic index or skin colour or any other biologically inherited characteristic".[26] We do not have to dwell on this flirtation with racialist ideas, but merely note here that most sociologists would now reject the notion of racially determined culture patterns on empirical grounds. However, in the present context, the importance of these statements is that they reveal Weber's uncertainty about explaining "a way of life common to whole groups of men", such as the protestant ethic. His reference to an explanation in terms of environmental conditions is paradoxical, for he makes it very clear in his methodological writings that he is primarily interested in *historical* explanations – and although he occasionally invokes factors such as the geographical environment, this is seen by him as a heuristic device along with the other modes of natural scientific analysis for the main business of meaningful explanation of unique historical sequences. As one scholar of Weber's works has recently put it: "Since he was concerned with the unique course of Western rationalization, he did not view it as a generic phenomenon ...".[27]

In a number of places however, Weber wrote of the process of rationalization as if it were an inevitable general "law of development":

> The increasing intervention of enacted norms is, from our point of view, only one of the components, however characteristic, of that process of rationalization and association whose growing penetration into all spheres of social action we shall have to trace

[25] Ibid., pp. 30, 31.
[26] Weber, *The Theory of Social and Economic Organisation* (1947), p. 85.
[27] R. Bendix & G. Roth, *Scholarship and Partisanship: Essays on Max Weber,* University of California Press, 1971, p. 114.

as a most essential dynamic factor in development. [28]

We have already seen how Weber believed that rationalization applied to many spheres of life in the occidental world and there are a number of other references to this process of general rationalization in his work, e.g. his statement in *The Methodology of the Social Sciences* that rationalization applies "not only to a history of philosophy and to the history of any other intellectual activity but ... to every kind of history".[29] He was careful however, as we have seen, to dissociate himself from metaphysical notions of history embodying "a group mind" or the development of the Hegelian "idea", as well as rejecting the natural scientific conception of analytical laws of development.[30] This rejection of laws of development can be seen in part as a legitimate objection to the tendency of reifying the process of rationalization into a metaphysical proposition – and Weber appears to have had Marx particularly in mind when he formulated this objection, as well as contemporaries of his such as Sombart.[31] But it is clear that Weber's position on this was also determined by his commitment to the historical cultural sciences and antagonism to naturalistic methodology.

However, Weber was forced by the logic of his own arguments to refer constantly to a "law of development" in order to explain the process of rationalization. His most explicit reference to this is contained in the footnotes appended to *The Protestant Ethic* and is made in the context of a discussion of economic determinism:

> ... religious ideas themselves simply cannot be deduced from economic circumstances. They are in themselves, that is beyond

[28] Weber, *Economy and Society,* Vol. I,, p. 333.
[29] Weber, *Methodology,* p. 34.
[30] Weber, *Methodology,* p. 101.
[31] Weber, *Methodology,* p. 103; Weber, *Protestant Ethic,* pp. 76, 77, 284.

> doubt the most powerful plastic elements of national character, and contain a law of development and a compelling force entirely their own.[32]

Weber refers to "autonomous laws" in other parts of his work[33] and even uses evolutionary terminology, e.g. in his sociological study of music he states that "rationalization proper commences with the evolution of music into a professional art" and this is only one of a number of references to evolutionary rationalization in the sphere of music.[34]

Although Weber was prepared to concede that any one historical development was the result of the interaction of a number of forces – economic, political, religious etc. – in practice his prime interest was in tracing the influence of religious rationalization. It is in this area of his work that he came nearest to formulating universal sociological principles:

> Scientific progress is a fraction, the most important fraction, of the process of intellectualization which we have been undergoing for thousands of years ... this intellectualist rationalization ... means that principally there are no mysterious incalculable forces that come into play, but rather that one can, in principle, master all things by calculation. This means that the world is disenchanted.[35]

This process of intellectualization is based on

> the metaphysical needs of the human mind as it is driven to reflect on ethical and religious questions, driven not by material need but by an inner compulsion to understand the world as a meaningful

[32] Weber, *Protestant Ethic,* pp. 277, 278.
[33] Max Weber, *The Religion of China,* Free Press, 1951, pp. 241, 249.
[34] Max Weber, *The Rational and Social Foundations* of *Music* (1958), pp. 41, 40, 106, 107.
[35] Gerth& Mills,op.cit.,pp. 138, 139.

cosmos and to take up a position towards it.[36] It is this rationalization of metaphysical ideas that presumably constitutes the law of development of religious ideas referred to above.

Although this law of development appears at first sight to be an example of a non-naturalistic "idealistic" law, there is no reason why if it is stated in appropriate language it should not be accepted as a proper scientific proposition. Rationalization can be defined as a variable in continuum form which characterizes the process of social change; it is possible to see rationality as an emergent property of the human mind based on the biological structure of the human brain, a product of the process of natural selection during man's biological evolution. The theme of rationalization has played a dominant intellectual role since at least the period of the Enlightenment, and nearly all the classic theories of social change have either explicitly or implicitly invoked the principle.

Perhaps the most important sociological exponents of this principle other than Weber were Comte and Marx: Comte used the principle and applied it to a notion of general cultural development primarily at the level of ideas; Marx applied it to developments of technology and the means of production. As we have seen. Weber himself was primarily interested in the rationalization of man's need to understand the meaning of his life at a metaphysical level – and these very metaphysical questions were seen by him even in the first instance, as a function of rationality itself.[37] None of these theorists satisfactorily answer the fundamental question as to why rationalization takes place in one society rather than another – in Weber's case of course the question being why did it develop so markedly in the occidental world and not elsewhere.

This argument about Weber's use of the concept of

[36] Weber, *Sociology of Religion*, pp. 116, 117.
[37]. Ibid., pp. 3, 6..

rationalization does not mean that he had abandoned an attempt to overcome the "naturalistic dogma". As we have seen, he did not recognize the law of development of rationalization as being a natural scientific proposition, and it is clear that his neo-Kantian voluntarism profoundly influenced his analysis of the development of the protestant ethic. In his *General Economic History* he wrote:

> In all times there has been but one means of breaking down the power of magic and establishing a rational conduct of life; this means is great rational prophecy.[38]

And a prophet according to Weber was "a purely individual bearer of charisma"[39] – and "charisma knows only inner determination and inner constraint".[40] Frequently Weber writes of charisma as if it were the source of the deep personal individual freedom that he admired so much; other times he sees it as a function of irrational forces often of a biological nature. The association of charisma with irrationality is seen by him as leading to unfreedom – and freedom here is seen as a function of a rationally developed ethic. This contradiction is the result of a marked ambivalence on Weber's part towards both rationality and charisma which come to have a different significance depending on the context in which he is using them.

The two forces of reason and charisma between them account for all the most important historical and social changes:

> In traditionally stereotyped periods, charisma is the greatest revolutionary force. The equally revolutionary force of "reason" works from without by altering the situations of action, and hence

[38] Max Weber, *General Economic History*, Collier Books, 1961, p. 265.
[39] Weber, *Sociology* of *Religion,* p. 46.
[40] S. N. Eisenstadt (ed.), *Max Weber on Charisma and Institution Building,* 1968, p. 20.

its problems, finally in this way changing men's attitudes towards them; or it intellectualizes the individual.[41]

There are obvious difficulties with this idea of charisma bringing about accumulative social changes. Inasmuch as the concept is used to refer to the profoundly personal creation of ultimate values,[42] all the logical objections to voluntaristic theories of action discussed earlier in the paper would apply. Charisma in itself will over a long enough period of time and from one social situation to another neutralize itself through a process of randomization, except where it is influenced by a socially structured set of influences. But pure charisma as such is an individual phenomenon and analytically must be sharply distinguished from socially determined facts. Of course it is possible to imagine a single individual's charisma being so powerful as to overwhelm all rival charismas, but this could only account for the influence of charisma on a limited single cultural situation defined by the immediate personal contacts of the charismatic leader. Any influence beyond this will be expressed through ideas and thus becomes subject to the principle of randomization in the absence of socially determined choices. Sociological facts of the stature of capitalist culture had to originate "not in isolated individuals alone, but as a way of life common to whole groups of men". In actual historical situations charisma is associated with the complete range of ideas and ethics, so that for example the charisma of the Protestant reformers no doubt can be matched by that of their Jesuit opponents.

The analysis of the development of the protestant ethic appears to contain equal emphasis on the role of both intellectual rationalization and charismatic innovation. The

[41] Ibid., pp. 53, 54.

[42] For an example of this see Weber's stress on Luther's personal experience and its importance for the disappearance of monasticism. Weber, *Protestant Ethic,* p. 121.

former refers basically to the level of ideas and changes in theological thinking; the latter to innovations in ethical doctrine propounded by the charismatic leaders of the Reformation. In this context it is easy to understand how many commentators on Weber's work have mistakenly assumed that ethical teaching was the major variable in the analysis. The question must be raised as to why Weber insisted that theological ideas had causal priority over ethical doctrine. The answer lies, I believe, in his uneasy awareness of the logical problems of voluntaristic explanations including those made in terms of charisma.

Of course the same problem could be raised with respect to theological ideas which can be said to also originate through the innovations of particular individuals. The difference is that developments of ideas can be classified according to the principle of increasing rationalization, whereas there is no obvious equivalent principle with which to classify changes in ethical doctrine. Weber did talk about the rationalization of ethical life, but although he is using the term rationalization here in a somewhat different sense to that used when applied to the level of ideas, in the last resort the concept returns the analysis back to the process of intellectual rationalization associated with the development of ideas.

It is now possible to understand why Weber not only gave priority to theological ideas in his analysis of the protestant ethic but also why he laid so much stress on Calvinist theology. According to Weber, Calvin's doctrine

> is derived not, as with Luther, from religious experience, but from logical necessity of his thought; therefore its importance increases with every increase in the logical consistency of that religious thought ... [43]

Logical consistency is one of Weber's main criteria of rationality and was viewed by him as the most important

[43] Ibid., p. 102.

characteristic defining theological rationality. It might be thought that he chose Calvinist theology as a key starting point of his analysis on empirical grounds, i.e. that he believed it to be empirically the most important of the theological doctrines that he considered. But Weber showed an uneasy awareness of a major problem in this part of his analysis:

> the types of moral conduct in which we are interested may be found in a similar manner among the adherents of the most various denominations ... similar ethical maxims may be correlated with very different dogmatic foundations ... It would almost seem as though we had best completely ignore both the dogmatic foundations and the ethical theory and confine our attention to the moral practice so far as it can be determined.[44]

Weber went on to reject this difficulty on empirical grounds, although he produced no evidence in any of his work to show that the Calvinists were any more thoroughly committed to the protestant ethic than any of the other Puritan groups with different theologies – such as the Arminian Quakers and Wesleyan Methodists. In fact a cursory examination of the evidence reveals that if anything the contrary is true and it is difficult to believe that Weber was unaware of this. If Calvinist theology was not chosen on empirical grounds – and Weber does not cite any evidence in support of this – it is likely that it was selected on theoretical grounds, specifically because of Weber's pre-occupation with finding out "whose intellectual child"[45] the protestant ethic was in terms of the dominant notion of rationalization.

The logical consistency of Calvinist theology was outlined by Weber in a brief passage in *The Protestant Ethic:*

> To assume that human merit or guilt play a part in determining; this destiny (of man) would be to think of God's absolute free decrees, which have been settled from eternity, as subject to

[44] Ibid., p. 97.
[45] Ibid., p. 78.

> change by human influence, an impossible contradiction ... His quite incomprehensible decrees have decided the fate of every individual and regulated the tiniest details of the cosmos from eternity.[46]

In other words, if God is viewed as being totally omnipotent and omniscient – as Christians have traditionally assumed – it is logically impossible by definition for him not to know the results of his creative activities before the actual creation of the universe. It is also by definition impossible for such a God to diminish his own power and transfer part of it to man in the form of free-will – such a transfer would limit his power, contradicting his total omnipotence. Weber's arguments about the psychological consequences of the Calvinist belief in predestination are very familiar and need only be touched on briefly here.

The Calvinist is faced with the problem of reconciling his need for salvation with his belief that it is impossible for him either to know or to be able to influence his salvation in any way. This creates acute metaphysical anxiety which is dealt with (this solution evolves over time) through using the ethical notion of success in one's "calling" as a "sign" of salvation. Weber goes to great pains to point out that this solution is a psychological not a logical one to the problems posed by a belief in predestination – according to him, the logical outcome is "fatalistic resignation" but the Calvinist does not follow this path because of his overwhelming need to "prove" himself in the face of his omnipotent God (the Calvinist's economic interests and social class position also predispose him to accept this illogical solution).[47] The doctrine of predestination creates a decisive psychological motive in the form of anxiety which is channelled into the active performance of a "calling" through the need of the

[46] Ibid., pp. 103, 104.
[47] Ibid., p. 232.

Calvinist to "prove" himself.

The doctrine of proving oneself before God was postulated by Weber as being common to all Puritan groups[48] – and inasmuch as it was a part of the "Christian ethic" it was a doctrine common to all Christians.[49] This however poses a problem in Weber's analysis, for on the one hand he states that the doctrine was a part of the "Christian ethic" and on the other that "the actual evolution to the proof of faith through works, which is the characteristic of asceticism, is parallel to a gradual modification of the doctrines of Calvin".[50] Implicit in the latter statement is the idea that the Calvinist's belief in predestination had somehow led to a natural development of evolving the doctrine of proof – yet this doctrine would have been associated with Calvin's original body of ethics as a part of the "Christian ethic". Weber's analysis could always be rescued from this objection by emphasizing the role of "practical interests" in determining the ethical consequences of the Calvinist's belief in predestination,[51] but this begins to shift the emphasis heavily away from a "spiritualistic" explanation towards an economic one.

Weber does however at one point relate the doctrine of proof to the mainstream of his sociological analysis:

> Grace could not be guaranteed by any magical sacraments, by relief in the confession, nor by individual good works. That was only possible by proof in a specific type of conduct unmistakably different from the way of life of the natural man. From that followed for the individual an incentive methodically to supervise his own state of grace in his own conduct, and thus to penetrate it with asceticism.[52]

This returns the discussion to the theme of rationalization – the

[48].Gerth & Mills, op. cit., p. 321.
[49] Weber, *Sociology* of *Religion,* p. 203.
[50] Weber, *Protestant Ethic,* p. 228
[51] Ibid., p. 232.
[52] Ibid., p. 153.

elimination of magical sacraments and religious ritual through the growth of scientific rationality. Weber distinguished a "subjectively rational" action from "one which uses the objectively correct means in accord with scientific knowledge".[53] Although he did not explicitly state that the elimination of magic is due to the growth of scientific rather than subjective rationality, this is implicit in his analysis, i.e. it is the development of a rational scientific emphasis on empirical observations rather than the internal logical rationalization of magic itself, which is important in its disappearance.

Weber believed that this process played a key role in cultural development:

> the complete elimination of salvation through the Church and the sacraments (in Puritanism) ... was what formed the absolutely decisive difference from Catholicism. That great historic process in the development of religions, the elimination of magic from the world which had begun with the old Hebrew prophets and, in conjunction with Hellenistic scientific thought, had repudiated all magical means to salvation as superstition and sin, came here (in Puritanism) to its logical conclusion. The genuine Puritan even rejected all signs of religious ceremony at the grave and buried his nearest and dearest without song or ritual in order that no superstition, no trust in the effects of magical and sacramental forces on salvation, should creep in.[54]

The consequence of the elimination of magic was that

> There was no place for the very human Catholic cycle of sin, repentance, atonement, release, followed by renewed sin. The moral conduct of the average man was thus deprived of its planless and unsystematic character and subjected to a consistent method for conduct as a whole.[55]

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[53] Weber, *Methodology,* p. 34.
[54] Weber, *Protestant Ethic,* pp. 105, 106.
[55] Ibid., p. 117.

This displacement of magic was not confined to any one Puritan denomination; according to Weber they were all equally affected by the process.[56] One of the most important features of the elimination of magic was the disappearance of the confessional: "it was a psychological stimulus to the development of their (the Puritans') ethical attitude. The means to a periodical discharge of the emotional sense of sin was done away with".[57]

Although Weber did not develop this theme about the psychological consequences of the disappearance of institutional magic, he made a number of isolated points which are capable of being formulated more systematically. One of the consequences of the diminution of the role of the church and its administration of sacred ritual was that the Puritan's "intercourse with his God was carried on in deep spiritual isolation"[58] and there "was a feeling of unprecedented inner loneliness".[59] The elimination of "the doctrine of salvation through the Church" culminated in the Quaker doctrine of the "significance of the inner testimony of the Spirit in reason and conscience".[60]

The final result of this process is that distinctive type of guilt and, so to speak, godless feeling of sin which characterizes modern man, precisely as a consequence of his organisation of ethics in the direction of a system based on an inner religious state, regardless of the metaphysical basis upon which the system was originally erected.[61] The similarity of this part of Weber's analysis with that made by Durkheim in *Suicide* is too striking to be ignored. The elimination of institutionalized magic and ritual activities was seen by

[56] Ibid., p. 119.
[57] Ibid., p. 106.
[58] Ibid., p. 107.
[59] Ibid., p. 104.
[60] Ibid., p. 149.
[61] Weber, *Sociology of Religion,* p. 206.

Durkheim as leading to an increase in the rate of "egoistic suicide" – an increase due to a decline in the amount of integration between the Protestant individual and his religious institutions (using. this term to refer to both belief and activity). Integration protects the individual from excessive reliance on himself which when carried to the extreme produces deep feelings of melancholy and eventually suicide. Weber and Durkheim disagreed about the role of intellectual rationalization in bringing about these results: Durkheim saw the intellectualism of the "egoist" as a by product of general social disintegration rather than as a causal factor in the process. Neither Weber nor Durkheim gives an adequate account of how religious institutions function to protect individuals from these feelings of anxiety, guilt and depression, for they both lacked a satisfactory psychological framework necessary to achieve such an explanation.

Although Weber's interpretations of social psychological situations are couched exclusively in ordinary language, it is possible to trace a set of psychological assumptions about the nature of the protestant ethic which are very similar to the postulates of psychoanalysis. When discussing puritan attitudes towards sport Weber wrote:

> Sport was accepted if it served a rational purpose, that of recreation necessary for physical efficiency. But as a means for the spontaneous expression of undisciplined impulses, it was under suspicion; and in so far as it became purely a means of enjoyment, or awakened pride, raw instincts or the irrational gambling instinct, it was of course strictly condemned. Impulsive enjoyment of life, which leads away from work in a calling and from religion, was as such the enemy of rational asceticism ... [62]

The contrast between rational self-control on the one hand and the irrational acting out of impulses on the other is very similar to the distinction made by Freud between the super-ego and

[62] Weber, *Protestant Ethic,* p. 167.

the id. The similarity is perhaps more clearly revealed by a comment by Weber on the relationship between the protestant ethic and sexuality:

> Rational ascetic alertness, self-control, and methodical planning of life are seriously threatened by the peculiar irrationality of the sexual act, which is ultimately and uniquely unsusceptible to rational organisation.[63]

The language used by Weber in these passages reveals a meaning of the word "rational" which extends that already discussed in connection with intellectual rationality: ethical rationality is the equivalent of the constraint of biological and emotional impulses which by their very nature threaten the deliberate and conscious reflection of intellectual rationality. From the other side, intellectual rationality is in part responsible for the suppression of sexual spontaneity; historically there had been a gradual turning away from the naive naturalism of sex. The reason and significance of this evolution, however, involve the universal rationalization and intellectualization of culture.[64] Weber saw the results of this "turning away from the naive naturalism of sex" in very much the same way as did Freud: the sublimation of sexual energy into work and rationality. Weber summarized his position when writing that

> the rejection of all naive surrender to the most intensive ways of experiencing existence, artistic and erotica, is as such only a negative attitude. But it is obvious that such a rejection could increase the force with which energies flow into rational achievement, both the ethical as well as the purely intellectual.[65]

Weber (like Freud) was ambivalent about this process of sublimation of sexual and emotional energy, for rationality can proceed in a variety of directions; positively in that of a

[63] Weber, *Sociology* of *Religion,* p. 238.
[64] Gerth & Mills, op. cit., p. 344.
[65] Ibid., p. 350.

conscious rationalization of ultimate values; or negatively, at the expense not only of custom, but of emotional values.[66] It was presumably these negative consequences that led Weber to view "intellectualism as the worst devil" [67].

The characteristics of the protestant ethic – "rational ascetic alertness, self-control, and methodical planning of life" – are not according to Weber confined specifically to a religious context, but are also the ethical qualities included in the definition of the secularized spirit of capitalism. The title of Weber's thesis is rather misleading in this respect: it suggests that the protestant ethic is a causally significant determinant of the independent spirit of capitalism, but it is clear from his methodological writings that they do not have a "determinate" relationship but rather have a "measure of inner affinity".[68] The spirit of capitalism is nothing but a more secularized version of the protestant ethic which develops over time through the process of rationalization. Perhaps this is revealed most clearly in Weber's summary of the nature of the spirit of capitalism:

> the *summum bonum* of this ethic, the earning of more and more money, combined with the avoidance of all spontaneous enjoyment of life, is above all completely devoid of any eudaemonistic not to say hedonistic, admixture ... it expresses a type of feeling which is closely connected with certain religious ideas.[69]

Weber went to great pains to dispel the idea (which some of his critics had mistakenly attributed to him) that the spirit of capitalism was the same thing as acquisitiveness and greed for gain:

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[66] Weber, *Theory of Economic and Social Organisation,* p. 112.

[67] There is some evidence that Weber failed to consummate his marriage because of sexual impotence. See Mitzman, op. cit., p. 276

[68] Weber, *Economy and Society,* p. xxxviii.

[69] Weber, *Protestant Ethic,* p. 53.

> Unlimited greed for gain is not in the least identical with capitalism, and still less its spirit. Capitalism *may* even be identical with the restraint, or at least a rational tempering of this irrational impulse.[70]

The language of this passage – "the restraint, or at least a rational tempering of this irrational impulse" – indicates the identical ethical and psychological nature of the protestant ethic and the spirit of capitalism. Both essentially are ethics which oppose what Freud called the pleasure principle and institutionalize ego and super-ego psychological forces. Weber does however qualify this point about acquisitiveness in stating that the Puritans did not struggle against rational acquisition, but against the irrational pursuit of wealth.[71] The result of this ethic was that

> When the limitation of consumption is combined with the release of acquisitive activity, the inevitable practical result is obvious: accumulation of capital through ascetic compulsion to save.[72]

The combined results of the "compulsion to save" and diligent activity in a calling led, in interaction with economic and other forces, to the development of modern capitalism.

Although the overwhelming emphasis of Weber's empirical analysis is on the causal influence of religious forces on economic development, he did also discuss the effect of economic factors on religious ideas and ethics. He explicitly stated that he believed this latter type of causal relationship to be of great importance:

> For those to whom no causal explanation is adequate without an economic (or materialistic as it is unfortunately still called) interpretation, it may be remarked that I consider the influence of economic development on the fate of religious ideas to be very

[70] Ibid., p. 17.
[71] Ibid., p. 171.
[72] Ibid., p. 172.

important.[73]

Weber's references to the economic determination of religious ideas are to be found scattered in rather piecemeal fashion in a number of his works. He located the protestant ethic in a Christian tradition associated distinctively with an urban status group of craftsmen and small traders:

> The wandering craftsman first appears at the beginning of our era. Without him the spread of Christianity would have never been possible; it was in the beginning the religion of the wandering craftsmen, to whom the Apostle also belonged, and his proverb "he who does not work shall not eat" expressed their ethics.[74]

Not only was this social group associated with the birth of Christianity, but during the Middle Ages it "remained the most pious, if not always the most orthodox, stratum of society".[75] It was the same group who formed the backbone of puritanism:

> With great regularity we find the most genuine adherents of Puritanism among the classes which were rising from a lowly status, the small bourgeois and farmers.[76]

Weber gave a number of reasons as to why this social group should be so predisposed towards puritanical Christianity. Primary among these reasons was the personal economic self-interest contained in the ownership of small amounts of property:

> The appropriation of the means of production and personal control, however formal, over the process of work constitute among the strongest incentives to unlimited willingness to work. This is the fundamental basis of the extraordinary importance of small units in agriculture, whether in the form of small-scale proprietorship or small tenants who hope to rise to

[73] Ibid., p. 277 fn 84.
[74] Weber, *General Economic History,* p. 111.
[75] Weber, *Sociology of Religion,* p. 95.
[76] Weber, *Protestant Ethic,* p. 174.

the status of owner.[77]

The acquisition of wealth destroys this ethic of work among this lower-middle class group; Weber illustrated this point by quoting Wesley's famous statement that "wherever riches have increased, the essence of religion has decreased in the same proportion".[78] The other major reason for the puritanism of this stratum lay according to Weber in its elimination of magical and traditional styles of thought (we have already discussed the ethical consequences of this intellectual development) – and this process of rationalization was essentially a function of the urban style of life of the lower-middle classes:

> When one compares the life of a lower-middle class person, particularly the urban artisan or the small trader, with the life of the peasant, it is clear that middle class life has far less connection with nature. Consequently, dependence on magic for influencing the irrational forces of nature cannot play the same role for the urban dweller as for the farmer. At the same time, it is clear that the economic foundation of the urban man's life has a far more rational essential character, viz., calculability and capacity for purposive manipulation.[79]

Weber's willingness to consider economic explanations is further illustrated by his position on the relationship between science and the process of rationalization: in his essay on science he summarized this when stating that "intellectualist rationalization" had been "created by science and scientifically oriented technology".[80] It is here that we see Marx's greatest influence over Weber. The location of religious ideas and ethics in an economic context does not however solve the fundamental problem that Weber set out to solve: "the special peculiarity of Occidental rationalism". Neither the

[77] Weber, *Theory of Economic and Social Organisation,* p. 242.
[78] Weber, *Protestant Ethic,* p. 175.
[79] Weber, *Sociology of Religion,* p. 97.
[80] Gerth & Mills, op. cit., p. 139.

emphasis on intellectualist or economic rationalization can explain why it was in the occidental world that rationality developed particularly in either or both these spheres. As we have seen Weber attempted to give an historical answer to the problem but raised a further difficulty which he never resolved: in criticizing a Marxist speaker at the first meeting of the German Sociological Association, Weber revealed his own position on the nature of historical explanation:

> I would like to protest against the statement made by one of the speakers that some one factor, be it technology or economy, can be the "ultimate" or "true" cause of another. If we look at the causal lines, we see them run, at one time, from technical to economic and political matters, at another from political to religious and economic ones etc. There is no resting point.[81]

It is for this reason that he accepted that in the analysis of cultural phenomenon "the appearance of the result is, for every causally working empirical science determined not just from a certain moment but "from eternity".[82]

This infinite causal regress is clearly a very unsatisfactory mode of explanation, for in the last resort it explains both everything and nothing. Although in principle Weber was prepared to accept that causal explanation could be regressed infinitely, in his substantive work on the development of the protestant ethic he was "not primarily interested in the origin, antecedents, or history of these ascetic movements, but (took) their doctrines as given in a state of full development".[83] It must be asked what principle enabled Weber to decide the point of departure for his analysis. In practice it

[81] Bendix & Roth, op. cit., p. 242.
[82] Weber, *Methodology,* p. 187.
[83] Weber, *Protestant Ethic,* p. 220. Weber did however make a number of substantive references to earlier historical developments and stated elsewhere that the "causal regress" of "present-day Christian capitalistic culture" might have to extend back "into the Middle Ages or Antiquity". Weber, *Methodology,* p. 155.

was the principle of understanding which allowed him to meaningfully explain the "inner affinity" of the protestant ethic with the spirit of capitalism. The function of understanding in empirical causal analysis was "to establish the really decisive motives of human actions",[84] and to enable Weber to break into the "eternal stream" of history for a point of departure of analysis. This point is necessarily a subjective rather than a material factor of analysis: Weber's methodology inescapably involved the understanding of subjective meanings. Material circumstances cannot be "understood" – a statement about them can only be invoked on Weber's methodology as a subsidiary heuristic device. The selection of puritan theology and the protestant ethic as a point of departure for Weber's analysis of the emergence of modern capitalism is therefore an example of a deeply partisan idealistic methodology.

The fundamental analytical problem that Weber set out to solve thus remains unanswered: what were the sociological factors responsible for the pervasive and systematic rationalization of occidental culture? Clearly Weber's references to a racial explanation of this cultural development form no basis whatsoever for a solution to this problem (the development of Japanese capitalism is by itself sufficient to discredit this purely speculative notion). Its solution lies far beyond the scope of this paper, although it is intended to return to this question in future work.

Weber's great achievement was to analyse the relationship between the disenchantment flowing from the process of rationalization and the evolution of the protestant ethic. This involved the sublimation of anxiety and guilt resulting from the destruction of protective belief and

[84] Weber, *Methodology,* p. 14. This notion that it is possible "to establish the really decisive motives of human actions' is reflected in Weber's conclusion that "the real roots of the religious ethics which led the way to the modern conception of a calling lay in the sects and heterodox movements, above all in Wyclif". Weber, *Protestant Ethic,* p. 203.

institutional magic (e.g. the elimination of the confessional), into the rationalized, methodical and sober ethic associated with both puritanism and certain aspects of occidental capitalism. Further work is required to elaborate the nature of the psychological forces that were involved in this process and why they took the form that they did. Although the protestant ethic has come to influence cultures outside of its area of origin, the question raised by Weber for comparative sociology still remains: why did the process of rationalization first develop in Western Europe, and not elsewhere?